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NOTICE!!

Effective September 1, 1991 all new manuscripts should be submitted to the new managing editor, Professor G. Bingham Powell, Department of Political Science, University of Rochester, Rochester, New York 14627.

In Memoriam

R. Taylor Cole

Managing Editor
American Political Science Review
1950–1952

Died 15 May 1991

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ARTICLES

RELATIVE GAINS AND THE PATTERN OF INTERNATIONAL COOPERATION

DUNCAN SNIDAL
University of Chicago

Many political situations involve competitions where winning is more important than doing well. In international politics, this relative gains problem is widely argued to be a major impediment to cooperation under anarchy. After discussing why states might seek relative gains, I demonstrate that the hypothesis holds very different implications from those usually presumed. Relative gains do impede cooperation in the two-actor case and provide an important justification for treating international anarchy as a prisoner's dilemma problem; but if the initial absolute gains situation is not a prisoner's dilemma, relative gains seeking is much less consequential. Its significance is even more attenuated with more than two competitors. Relative gains cannot prop up the realist critique of international cooperation theory, but may affect the pattern of cooperation when a small number of states are the most central international actors.

Recent international theorizing continues the long-standing contest between realists who argue that prospects for international cooperation are quite limited and institutionalists who argue that cooperation is possible among states. At the risk of caricature, the relevant strands of this debate can be stated simply. Since Thucydides, realists have proposed that international anarchy makes cooperation difficult because agreements cannot be centrally enforced. In the past decade, institutionalists have found their countervailing case greatly strengthened by analytical results demonstrating, under plausible circumstances, the possibility of decentralized enforcement of cooperation. Recently, realists have replied that this result does not apply to international politics because it is a realm where states seek relative gains, rather than the absolute gains analyzed by institutionalists. I examine this last claim analytically to determine the implications of relative gains for international cooperation.

Relative gains do not provide a sufficient response to the institutionalist claim about cooperation. Only in the very special case of the two-state interaction, with high concern for relative gains and near disregard for absolute gains, is the realist case compelling. For a broad range of more realistic problems, where there are more than two states or where states care about a mixture of absolute and relative gains, the institutionalist case for the possibility of decentralized cooperation remains strong.

My results have a broader relevance for other important social and political settings. Whenever individuals seek status or victory, whenever they engage in contests or tournaments, and whenever goods are "positional" in nature, relative gains are at stake. Since international anarchy, where states seek power or security, is often compared to a Hobbesian state of nature where individuals seek eminence or safety, my results on international cooperation apply equally to contractual theories of the state. Relative gains analy-

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sis may also contain insights for understanding certain aspects of bureaucratic or electoral competition. I leave the exploration of these general similarities to the reader, however, and focus instead on relative gains in the specific context of international politics.

I begin with a brief discussion of why realists believe states seek relative gains. Next I examine the strongest part of the realist case concerning the impact of relative gains in interactions involving two states. When two states care only about relative gains, their relations can be modeled as a zero-sum game with no room for cooperation. When states are largely, though not exclusively, motivated by relative gains, their relations are shown to be equivalent to the prisoner's dilemma (PD) regardless of the structure of the underlying absolute gains game. This supports the standard realist construction of international anarchy as PD and suggests why cooperation is problematic. If the initial absolute game is not PD, however, the relative gains PD will be fairly mild and susceptible to decentralized cooperation. But if the absolute gains game is already PD, then incorporating relative gains considerations makes this PD more intense and decentralization cooperation more difficult. In effect, I establish the limited circumstances under which relative gains incentives turn two-actor international situations into either zero-sum games or such intensely conflictual PDs that cooperation is well-nigh impossible.

Relative gains do not matter nearly so much in less restrictive, more realistic circumstances. I develop a model to examine relative gains in PD situations with more than two actors. The general result is that *a small increase in the number of actors dramatically decreases the impact of relative gains in impeding cooperation.* This effect is augmented by interaction with anything less than a total concern for relative gains. The realist case is thereby shown to be quite weak outside the pure relative gains, tight bipolar world.

Finally, I explore the implications of relative gains for international politics. Although relative gains do not eliminate cooperation, they do predict specific patterns for international cooperation. These predictions provide novel explanations for standard "stylized facts," including why the small exploit the large, why cooperation leads to hegemonic decline, and why particular patterns of cooperation prevail in a bipolar world. Other implications address long-standing disputes in the field, including a surprising finding that relative gains advocates should prefer multipolarity to bipolarity. While the analytical basis for these specific conclusions deserves further refinement, they illustrate the testable propositions that would allow us to evaluate the relative gains hypothesis as an explanation of international behavior.

Thus, the relative gains hypothesis cannot salvage the general pessimism of realism but has interesting implications deserving further careful analytical and empirical investigation. It clearly has important consequences for two-actor situations and, where there are small numbers or important asymmetries among larger numbers, it may modify conclusions obtained from the absolute gains model. In this way the relative gains argument may find an important place in international relations theory.

Why Assume That States Seek Relative Gains?

The assumption that states seek relative gains influences a wide variety of scholarship on international politics. A number of scholars have discussed the inhibiting effect of relative gains on cooperation (Gilpin 1981, 1987; Gowa 1986; Grieco 1988a, 1988b, 1990; Jervis 1988; Kennedy 1987; Krasner 1987; Lake 1984; Larson

1988; Lipson 1984; Stein 1982; Waltz 1979). Realists argue that the general insecurity of international anarchy leads states to worry not simply about how well they fare themselves (absolute gains) but about how well they fare compared to other states (relative gains). States that gain disproportionately in relations with other states may achieve a superiority that threatens the goals or even the very security of their cooperative partners. Kenneth Waltz summarizes this perspective well:

> When faced with the possibility of cooperating for mutual gain, states that feel insecure must ask how the gain will be divided. They are compelled to ask not "Will both of us gain?" but "Who will gain more?" If an expected gain is to be divided, say, in the ratio of two to one, one state may use its disproportionate gain to implement a policy intended to damage or destroy the other. Even the prospect of large absolute gains for both parties does not elicit their cooperation so long as each fears how the other will use its increased capabilities. (1979, 105)

Thus, states are seen to seek relative gains and the inference is drawn that this inhibits cooperation.

The relative gains hypothesis applies to economy as well as security. In part, this is because economic gains can ultimately be transformed into security gains, so that in the long run, security and economics are inseparable. But relative gains thinking occurs independently in economics. Pure mercantilism, where states seek to accumulate specie, entails relative gains aspirations that pit state against state. Similar implications emerge from neomercantilism and the new theory of "strategic trade," with its emphasis on how economies of scale in industries present states with incentives to interfere in trade to gain market shares. This suggests that the pursuit of relative gains can interfere with economic as well as military cooperation.

I do not address the plausibility of relative gains seeking as a description of international behavior, I stipulate it as an assumption. My reason is that I am ambivalent as to how much such aspirations or fears motivate states. I do not believe that relative gains pervade international politics nearly enough to make the strong realist position hold in general. In some circumstances, however, relative gains can help explain certain aspects of international behavior. Rather than debate its merits a priori, I evaluate the relative gains assumption by exploring its implications, which are not yet correctly understood. By accepting the assumption of relative gains maximization, I show that it does not have the general inhibiting effect on international cooperation widely ascribed to it.

Relative gains seeking can inhibit cooperation in two ways. The less important way is by limiting the range of viable cooperative agreements because states will not accept deals that provide disproportionately greater benefits to others.[1] This understanding of relative gains seeking does not clearly distinguish relative from absolute gains. Intense bargaining for greater absolute benefits also leads to a concern with the distribution of joint gains from cooperation. Indeed, in the pure zero-sum case there is no analytic or substantive difference between seeking a greater absolute amount and seeking a greater relative share. More importantly, if distribution is the primary relative gains problem, states can alter the terms of a cooperative agreement or offer side payments until the distribution of gains is sufficiently proportionate.[2] Ironically, this relative gains problem might even facilitate cooperation by narrowing the range of viable cooperative agreements and thereby reducing the absolute gains bargaining problem.[3]

Therefore, I focus on the second, more general way in which relative gains affect international cooperation, namely, by changing states' incentives.[4] Common interests created by the prospect of joint absolute gains become increasingly conflictual as relative comparisons are introduced. This alters the strategic structure of interstate relations and thereby decreases the prospects for cooperation. As I shall show, even purely harmonious absolute gains situations between two actors approximate zero-sum conflictual contests when relative gains are important. If room for cooperation remains, agreements are often less viable, since states' incentives to violate them increase under relative gains. Thus, relative gains decrease states' interests in cooperation as well as their ability to maintain self-enforcing agreements in anarchy.

It should be noted that the hypothesis of relative gains seeking can be criticized as a misspecification of an argument that could be better expressed in absolute gains terms. When Waltz argues that states do not cooperate when others will gain more than them and threaten their security, he is implicitly describing a trade-off between short-term absolute gains (i.e., immediate payoffs from cooperation) and long-term absolute gains (i.e., security over the long haul). This choice problem is perfectly amenable to absolute gains analysis including effects over time, without recourse to relative gains. Similarly, if states forgo immediate economic benefits to capture market share and score relative gains, their ultimate goal is monopoly profits as a long-run absolute benefit of their short-run sacrifice. Even if competition among states prevents any of them from realizing these long-run absolute benefits, that does not diminish the importance of these incentives in motivating states. Thus, it is possible to substitute a more complicated absolute gains maximization problem for the relative gains hypothesis (Powell 1990).

There are several reasons why it makes sense to investigate implications of the prevailing relative gains hypothesis rather than translate it into absolute gains terms. First, despite the foregoing comments, perhaps states really do care about relative gains independent of these other considerations. To the extent that goals such as prestige, status, or winning motivate states, relative gains are basic elements of their preferences.[5] Second, relative gains may serve as a useful proxy for state goals just as in the realist's view power may serve as a proxy for states' more specific goals (Morgenthau 1978). Finally, because realists have failed to produce a model presenting the goals of states in a more explicit and complete way, I am loath to undertake that task for them. Any alternative model I proposed could be rejected as not a fair or true depiction of their arguments. Therefore, I take the relative gains hypothesis as it stands and use a specification for relative gains that is analytically equivalent to Grieco's (1990, 41).

Relative Gains with Two States

Relative gains considerations convert a wide variety of absolute gains situations into PD. Since PD is the typical depiction of the international anarchy associated with relative gains, this may not seem surprising. But other strategic situations, including *chicken* and *assurance* games, are also often discussed in connection with international anarchy. Furthermore, liberals who challenge the realist conception of international anarchy sometimes argue that the circumstances of international politics (or at least some international issues) are best represented as *coordination* or even *harmony* games. Choosing among such different analytic representations consequently poses a tough problem at the foundations of international relations theory. The assumption of relative gains provides an escape from this quandary by offering a substantive motivation for the analytic presumption that PD is the archetypical international problem.[6]

Figure 1. Absolute Gains Game

	D	C
D	0, 0	F, U
C	U, F	M, M

Relative Gains Create Prisoners' Dilemmas

To investigate the impact of relative gains, consider a standard two-actor game matrix where each actor faces a decision to cooperate (C) or defect (D) from cooperation.[7] Payoffs are initially defined in absolute terms. If both states cooperate, each receives payoff M from *mutual* cooperation. If one state cooperates *unilaterally*, it receives payoff U while the other state receives the *free-ride* payoff F. If neither cooperates, each receives a zero payoff (0), as the normalized payoff associated with *noncooperation.* Figure 1 depicts these four possible outcomes in a simple game matrix.

Three substantively motivated restrictions on the payoffs focus the analysis on the most relevant subset of the 78 ordinal game situations implied by this two-by-two structure (Hardin 1983; Rapoport and Guyer 1966). First, assume that the game is symmetric (as is implicit in the absence of subscripts on the payoffs). This means that two states are equally well situated to benefit or hurt one another and fashion their cooperative agreement to provide equal absolute gains. Later I shall relax this assumption to examine interactions among multiple states of different sizes and with different relative gains concerns for one another.[8] Second, assume that states prefer mutual cooperation to mutual defection, so that $M > 0$. This follows from a commonsense understanding of what we mean by cooperation. It is not a significant restriction on the analysis, since it affects only naming conventions for behaviors C and D and not the forms of strategic interdependence allowed. Interchanging C and D for each state converts any game where $0 > M$ into a strategically equivalent game, with only the labels changed, where $M > 0$. Third, assume that states prefer a free ride over unilateral cooperation, so that $F > U$. This fosters individual incentives not to cooperate which, in conjunction with the previous assumption of a collective incentive to cooperate, results in an interesting set of cooperation problems. Since the contrary assumption $U > F$ typically facilitates cooperation, we are effectively limiting the analysis to cases where relative gains have a negative impact on cooperation.[9]

These three restrictions produce six strategic situations spanning the range of cooperation problems as shown in Table 1. One possible game is appropriately described as *harmony* because both sides have dominant strategies resulting in an equilibrium where each gets its maximal outcome. Two other possibilities correspond to versions of the *assurance* or *stag hunt* game, where one of two equilibria is mutually preferred. Although joint cooperation provides the best outcome for both, it is not guaranteed. Fear that the other will fail to cooperate may lead risk-averse actors not to cooperate and thereby to the deficient equilibrium outcome. A fourth possibility is a *coordination* game, where both states have incentives to coordinate on either of two equilibria but differ over which one to select.[10] Next is *chicken* where states differ strongly over which equilibrium is preferred, where a Pareto-optimal but nonequilibrium compromise outcome is available to cooperating states and disaster occurs in

Figure 2. Relative Gains Game

	D	C
D	0, 0	F-U , U-F
C	U-F, F-U	0,0

the form of the mutually worst outcome if neither cooperates. Finally, our three restrictions can also lead to PD where individually rational defection results in an equilibrium leaving states collectively worse off. This game turns out to be of increasing relevance with the introduction of relative gains considerations.

Corresponding to each of these absolute gains games is a relative gains game defined by the difference between actor's payoffs (Grieco 1988a, 1988b, 1990; Taylor 1987).[11] Figure 2 shows the payoff matrix for the pure relative gains model. The resulting game is zero-sum, with the same ordinal structure for all six absolute gains games. Since $F > U$, defection (D) is always a dominant strategy for both actors. Thus pure relative gains considerations eliminate all prospects for cooperation in two-actor strategic interactions.

Only in the most extreme case would we expect states to care only about relative gains and totally disregard absolute gains. A more realistic presumption is that states seek relative gains in combination with absolute gains, with the pure absolute and relative gains models seen as limiting cases. This continuum is represented by a weighting parameter r for the importance of relative gains, and $(1 - r)$ for the importance of absolute gains, where $0 \leq r \leq 1$. Larger r means that states care more about relative gains and less about absolute gains. Combining relative and absolute gains considerations in this way leads to the payoff matrix shown in Figure 3.[12]

This framework allows us to investigate the consequences of increased concern for relative versus absolute gains for the game structure. Recall the impact of relative gains in Figure 2. Relative gains provide a negative payoff for unilateral cooperation (since $F > U$), a positive payoff for free-riding, and zero payoff where states gain equally through either mutual coopera-

Figure 3. Mixture of Absolute and Relative Gains Games

	D	C
D	0,0	F*= F-rU , U*= U-rF
C	U*= U-rF , F*= F-rU	M*= (1-r)M , M*= (1-r)M

Table 1. Six Representative Cooperation Problems

Preference Ordering for Each Side	Ordinal 2 × 2 Game	Popular Name
$M > F > U > 0$	$\begin{array}{ccc} & D & C \\ D & 1,1 & 3,2 \\ C & 2,3 & \enclose{circle}{4,4} \end{array}$	Harmony
$M > F > 0 > U$	$\begin{array}{ccc} & D & C \\ D & \enclose{circle}{2,2} & 3,1 \\ C & 1,3 & \enclose{circle}{4,4} \end{array}$	Assurance I (stag hunt)
$M > 0 > F > U$	$\begin{array}{ccc} & D & C \\ D & \enclose{circle}{3,3} & 2,1 \\ C & 1,2 & \enclose{circle}{4,4} \end{array}$	Assurance II (stag hunt)
$F > U > M > 0$	$\begin{array}{ccc} & D & C \\ D & 1,1 & \enclose{circle}{4,3} \\ C & \enclose{circle}{3,4} & 2,2 \end{array}$	Coordination
$F > M > U > 0$	$\begin{array}{ccc} & D & C \\ D & 1,1 & \enclose{circle}{4,2} \\ C & \enclose{circle}{2,4} & 3,3 \end{array}$	Chicken
$F > M > 0 > U$	$\begin{array}{ccc} & D & C \\ D & \enclose{circle}{2,2} & 4,1 \\ C & 1,4 & 3,3 \end{array}$	Prisoner's Dilemma

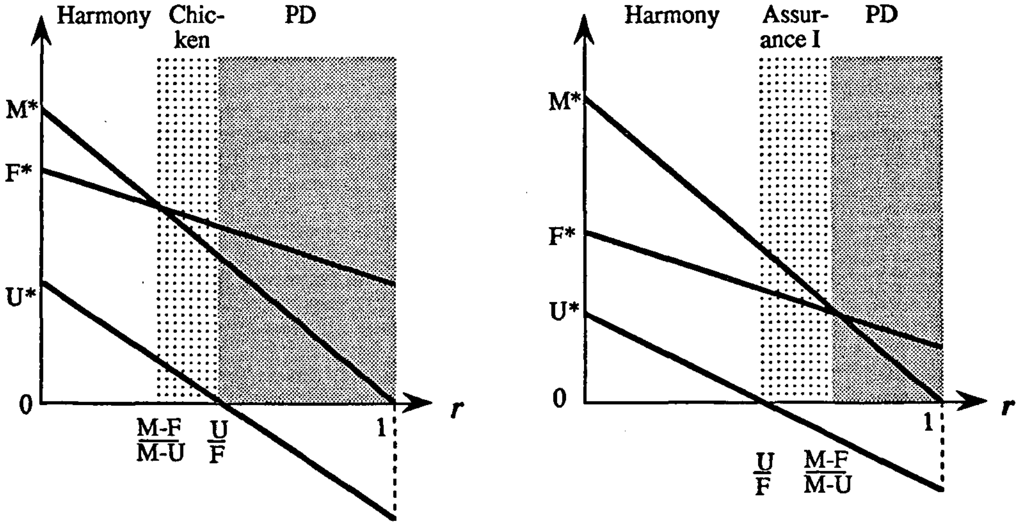
Note: The games are derived under assumptions of symmetry, that mutual cooperation is preferred to mutual defection, and that free-riding is preferred to unilateral cooperation. Payoffs are ordinal with higher numbers representing more preferred outcomes. They are not normalized to have mutual defection equal to zero as in the text. Nash equilibria are circled.

tion or mutual defection. In addition, increased emphasis on relative gains means that absolute payoffs are less important, so that the value of cooperation itself drops. These two effects produce transformed U^*, F^*, and M^* payoffs (where the asterisk represents the combination of absolute and relative gains considerations) while not affecting the normalized noncooperation payoff of 0. For example, increasing r decreases M^* unconditionally and decreases U^* provided $F > 0$. In turn, changes in these values change the rank order of states' preferences and thereby transform the game structure.

The transformation of the harmony game illustrates how relative gains calculations change game structures. The initial preference ordering for each state is $M > F > U > 0$. As relative gains considerations become more important, the first three payoffs in this ordering decrease but at different rates. When $U > M - F$, as in the left half of Figure 4, the harmony ordering is maintained until $r > (M - F)/(M - U)$. Beyond this, the preference orderings become $F^* > M^* > U^* > 0$, and the structure of the game is changed to chicken (see Table 1). If relative gains concerns increase further, so that $r > U/F$, the individual orderings become $F^* > M^* > 0 > U^*$ and structure of the game converts to PD. This structure persists until $r = 1$ and the pure relative gains, zero-sum game of Figure 2 pertains. Conversely, when $U < M - F$, as in the right half of Figure 4, the harmony ordering is maintained until $r > U/F$, at which point the structure of the game is transformed to assurance I. As r increases further, the structure of the game converts to PD once $r > (M - F)/(M - U)$. Again, this structure persists until $r = 1$.[13]

Only one scenario is possible for each of the other five symmetric games, as

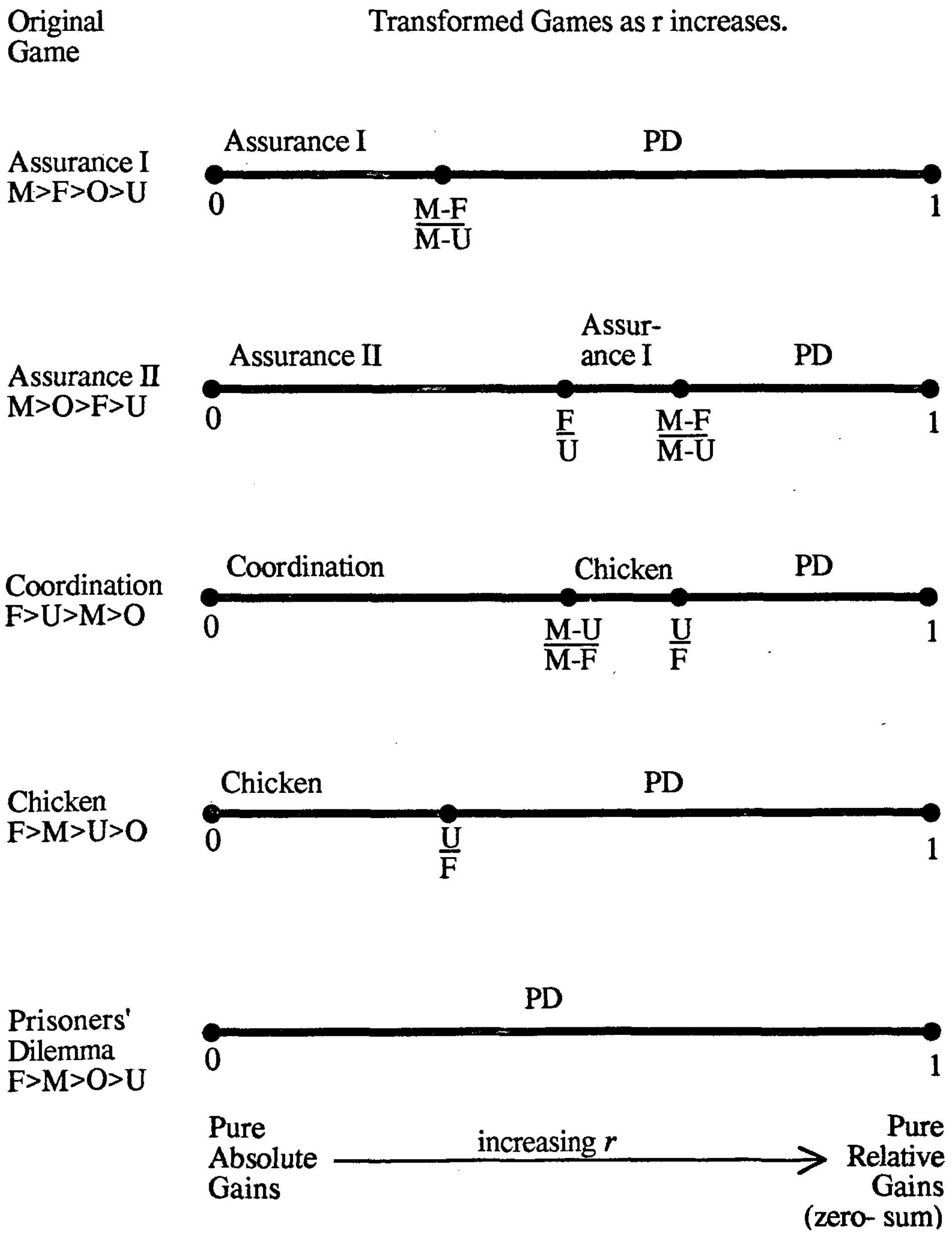
Figure 4. Different Relative Gains Transformation of Harmony to Prisoner's Dilemma



a. M>F>U>0 with U>M-F

b. M>F>U>0 with U<M-F

Figure 5. Relative Gains Transformation of Absolute Gains Games into Prisoners' Dilemmas


Original Game
Transformed Games as r increases.
Assurance I
M>F>O>U
Assurance I
PD
0
M-F
M-U
1
Assurance II
M>O>F>U
Assurance II
Assur-ance I
PD
0
F
U
M-F
M-U
1
Coordination
F>U>M>O
Coordination
Chicken
PD
0
M-U
M-F
U
F
1
Chicken
F>M>U>O
Chicken
PD
0
U
F
1
Prisoners' Dilemma
F>M>O>U
PD
0
1
Pure Absolute Gains
increasing r
Pure Relative Gains (zero- sum)

Note: Intervals for r (e.g., $F/U \leq r \leq (M - F)/(M - U)$) are not directly comparable across cases because the stipulated orders of M, F, U, and 0 are not the same. Prisoners' dilemmas become more intense toward the right.

shown in Figure 5. Assurance I is transformed into PD once the weighting of relative gains exceeds $(M - F)/(M - U)$. Assurance II passes through an intermediate phase, first being transformed into assurance I when $r > R/U$ and then into PD once $r > (M - F)/(M - U)$. The coordination game passes through an intermediate phase of chicken before becoming PD as r increases. Chicken is transformed directly into PD once $r > U/F$.[14] Finally, PD maintains its same structure for all relevant values of r.[15] However, this PD structure becomes more intense with increasing r, since the temptation to free-ride increases, the consequences of unilateral cooperation are more severe, and the advantages of mutual cooperation over mutual defection decrease.[16] In all situations, the limiting case where only relative gains matter (i.e., $r = 1$) is the zero-sum game where both sides have a dominant strategy not to cooperate.

This tendency to convert absolute gains situations into PD can be summarized in terms of the impact of relative gains motives on states' payoffs. Since $F > U$, $F^* = F - rU$ must become positive as r increases toward 1. Therefore, free-riding is eventually preferred to mutual cooperation (i.e., $F^* > M^*$), since $M^* = (1 - r)M$ becomes a progressively smaller positive payoff as relative gains become more important. Until $r = 1$, however, mutual cooperation provides a greater payoff than the normalized 0 payoff from mutual defection (i.e., $M^* > 0$). Finally, relative gains pressures necessarily make $U^* = U - rF$ negative and the least preferred payoff as r increases (i.e., $0 > U^*$). Thus, as r increases, the preference orderings of states are eventually transformed to $F^* > M^* > 0 > U^*$, which defines PD. In the limiting case where $r = 1$, the game becomes zero-sum with $F^* > M^* = 0 > U^*$ and $F^* = -U^*$. Whatever the initial absolute gains circumstance, relative considerations augment incentives for states not to cooperate for familiar reasons associated with PD and zero-sum games.[17]

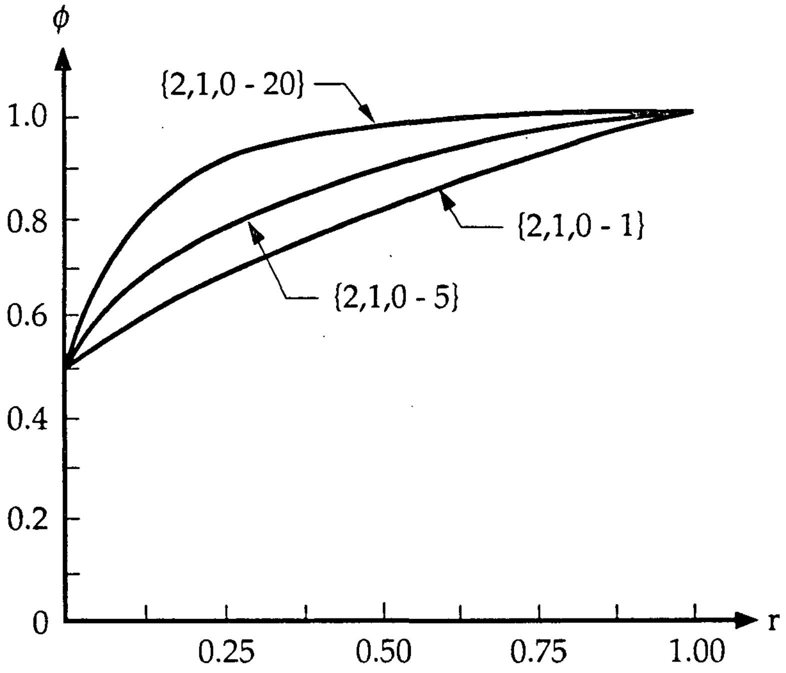
Thus, we see how the substantive intuition of realism that states care about relative gains maps into its implicit analytic presumption that international anarchy can be characterized as a PD. Relative gains calculations transform each of the various absolute gains circumstances presented in Table 1 into PD. This makes cooperation problematic even when absolute gains conditions are congenial as in harmony or assurance. The greater the concern for relative gains, the more intense is the resulting PD, until $r = 1$, when all two-actor interactions become zero-sum. In these circumstances, realists argue, cooperation is much more difficult or even impossible.

Prospects for Cooperation in a Two-Actor Relative Gains World

How much do relative gains considerations impede cooperation in a two-actor world? When states are exclusively relative gains seekers, the two-actor world is zero-sum and cooperation is impossible. Because such an extreme assumption is rarely warranted, I shall briefly examine the impact of more realistic, intermediate levels of relative gains aspirations. First, I consider how mixtures of relative and absolute considerations affect cooperation within PD. Second, I evaluate the tendency for relative gains aspirations to transform non-PD absolute gains games into PD. Finally, I measure the intensity of the resulting PDs. The conclusion is that moderate levels of relative gains reinforce the characterization of international politics as PD but that only quite high levels of r produce intense PDs from other absolute gains situations.

First, what is the differential impact of low, medium, and high levels of relative gains ambitions in PD? If small degrees of relative gains make any PD very intense, the deleterious implications for coopera-

Figure 6. The Relationship between ϕ and r for Three Prisoners' Dilemmas of Increasing Severity



Note: Numbers in parentheses correspond to {*F*, *M*, 0, *U*}.

tion are enormous. Conversely, if only an extreme relative gains preoccupation makes PD very intense, relative gains considerations are important only in rare circumstances.

Changes in the *minimum discount factor* ($0 \leq \phi < 1$) required to support cooperation provide a useful measure of the impact of relative gains in inhibiting cooperation in the PD. The discount factor measures the extent to which a state values future benefits relative to current benefits. For example, a discount factor of .8 means that a state values a unit of benefit in the next period as worth only .8 units in the current period and, generally, values a unit of benefit p periods in the future at only $.8^p$ units today. Since decentralized cooperation in PD can be supported by ongoing future incentives offered by mutual cooperation (e.g., through joint choice of tit-for-tat strategies), it is more likely to be stable when a lower discount factor is required of states. The reason is that smaller future benefits of continuing cooperation are then sufficient to outweigh larger immediate gains from noncooperation (Axelrod 1984). Minimum ϕ is the lowest discount factor for states at which cooperation is stable; increases in minimum ϕ indicate the deleterious impact of relative gains on cooperation. A related measure used below is the increase in minimum discount factor induced by a given level of relative gains seeking as a proportion of the total change in the discount factor caused by a shift from pure absolute gains to pure relative gains seeking.[18]

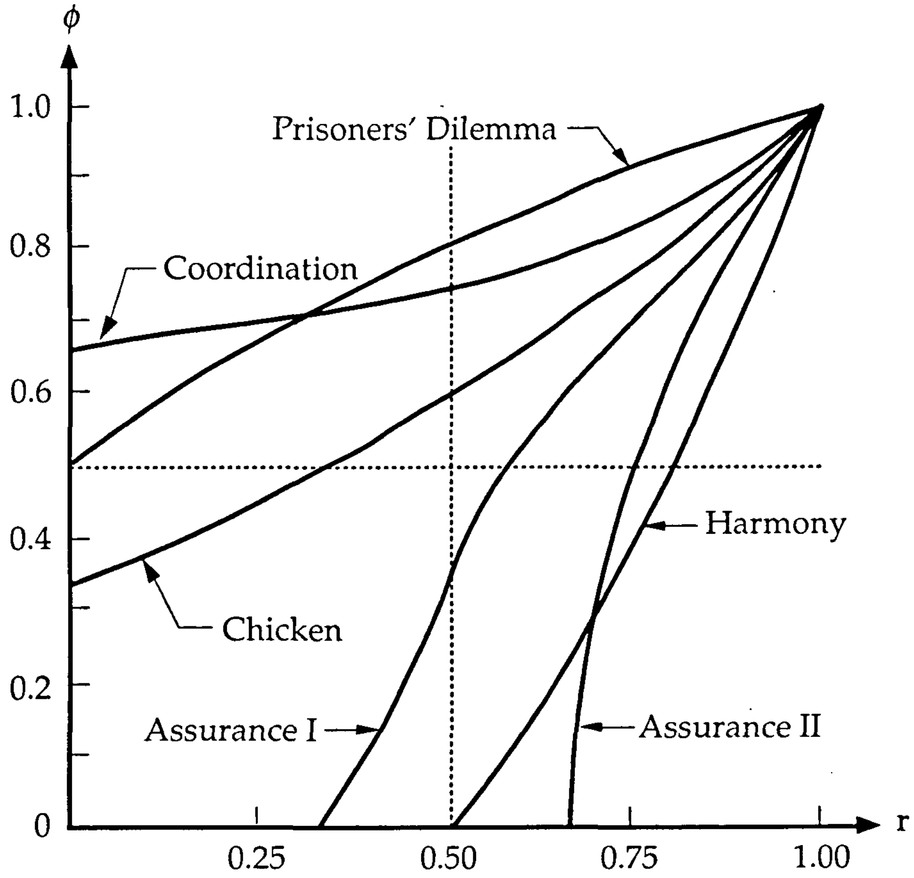
Figure 6 shows that cooperation in PD is most greatly affected when relative gains concerns increase from low levels (i.e., $\partial\phi/\partial r > 0$ and $\partial^2\phi/\partial r^2 < 0$). The top curve clearly illustrates this concave relation between r and ϕ. Here r has had nearly 80% of its impact on ϕ by the time $r = .25$, and over 90% by the time $r = .5$. This is an extreme case, however, where the difference between unilateral cooperation (U) and no cooperation (0) is 20 times as great as the difference between cooperation (M) and no cooperation (i.e., $\{F, M, 0, U\} = \{2, 1, 0, -20\}$). In general, low levels of r have their greatest impact when U is low compared to the other payoffs. The bottom curve presents a possibly more typical PD game based on the "standard" preference ordering (i.e., $\{4, 3, 2, 1\}$ normalized to $\{2, 1, 0, -1\}$). Here the impact of low levels of relative gains on ϕ is much more modest. Nevertheless, the proportional impact of low levels of relative gains is greater at lower levels of r with 33% of their impact on ϕ occurring for levels of $r = .25$, and 60% of their impact occurring when $r = .5$. The middle curve shows similar results for an intermediate case $\{2, 1, 0, -5\}$. These results indicate that small degrees of relative gains motivations are important, though not necessarily decisive, in impeding cooperation in PD games.

The second issue is how quickly relative gains transform other absolute games into PD. Since F, M, 0, and U can vary widely within the minimal constraints imposed on them, it is difficult to generalize as to the level of relative gains pressures necessary to convert various absolute gains games into PD. Very low levels of r are sufficient to transform different situations into PD whenever the payoff from unilateral cooperation (U) is significantly lower than the gains from free-riding (F). Conversely, when unilateral cooperation is almost as beneficial as free-riding (i.e., U is close to F) games become PD only when r is close to unity. And where mutual cooperation is preferred to free-riding, low levels of relative gains considerations inhibit cooperation only if the difference (i.e., $M - F$) is small. Most importantly, it is obviously easy to concoct particular examples where small or large doses of relative gains are needed to have an impact.

A way to appreciate the impact of relative gains is in terms of the "standard" two-by-two game preferences with the best outcome equal to four and the worst outcome equal to one. These interval-level payoffs are normalized so that mutual noncooperation is equal to zero. In this case, assurance I and chicken turn into PD when $r > 1/3$; harmony turns into PD when $r > 1/2$ and assurance II and coordination turn into PD when $r > 2/3$. The PD, of course, remains PD throughout but becomes more intense with increasing r.[19] We can conclude that under the typical specification of two-by-two game payoffs, a moderate (i.e., 1/3) to high (i.e., 2/3) level of r is necessary to transform non–PD games into PD.

An important caveat follows: *PD results significantly overstate the deleterious impact of relative gains on cooperation in other absolute gains circumstances.* The reason is that the initial impact of relative gains is used up in first converting these other games to PD. Only then can increasing relative gains pressures make the resulting PD progressively more intense. Figure 7 shows these differential results for the standard preference rankings described. Consider a comparison of the minimal discount factor (ϕ) required to support cooperation in various games when $r = .5$. Cooperation in PD requires $\phi > .8$, whereas chicken requires $\phi > .6$ and assurance I requires $\phi > .33$. The cooperative equilibrium in harmony is stable for any positive discount factor when $r = .5$, since it is at this point that it has just converted to PD.[20] Assurance II possesses a stable cooperative outcome for $r < 2/3$ and is not greatly affected by relative gains considerations of this magnitude. Indeed, except for

Figure 7. The Relationship between ϕ and r for Six Standard Games



a minor exception in the coordination game, the minimum discount factor (ϕ) required for a stable cooperative equilibrium is higher for PD than for the other games until $r = 1$. Therefore, relative gains findings for PD significantly overstate their impact on other initial absolute gains games. Relative gains make other games PD, but not especially intense ones.[21]

Thus, the intuition of realism is confirmed but qualified whenever there are substantial relative gains concerns between two states. Although relative gains transform other absolute gains situations into PD, PD results significantly overstate the impact of relative gains for these other games. Within PD, the impact of increasing relative gains is greatest at low levels of r and increases at a decreasing rate until all prospects for cooperation are eliminated at $r = 1$. This lends some credence to the realist conclusion that cooperation is more difficult when states are relative gains seekers and are in absolute gains PD situations.

Because this realist pessimism is predicated on the two-actor case, it cannot be uncritically transferred to the multiactor world—as it has been—without careful scrutiny. I now stipulate that the initial absolute gains situation is PD—the most favorable assumption for the relative gains case—and examine the further limitations to the realist position when there are more than two states and they care about both relative and absolute gains.

Relative Gains with More Than Two States

A separate paper analyzes cooperation among different numbers of identical states that care only about relative gains (Snidal 1991). It shows that relative gains do *not* affect prospects for cooperation whenever the number of states (n) is large. Because those results depend on large n, however, that analysis is inconclusive as to the effect of relative gains involving small numbers of states. It also assumes symmetry (in that states are of equal importance) and does not allow for mixtures of relative and absolute gains objectives.

I explore here the impact of relative gains among small numbers of states, especially when they are not all identical. The logic is to examine the conditions under which cooperation is a stable equilibrium outcome in an iterated relative gains PD, using the minimum discount factor (ϕ) as an indicator of the difficulty of cooperation. This equilibrium is examined to see how it is affected by variations in overall concerns for relative gains as well as differential concerns with relative gains vis-à-vis particular other states. Because the analysis is generalized to include states of different sizes, I shall first examine how the costs and benefits of cooperation are distributed among states of different sizes.

Why Cooperative Gains Are Equally Distributed between States

The central result derived is that small and large states share equally in costs and benefits of international cooperation; that is, when Canada and the United States cooperate, each bears equal costs and earns equal benefits even though Canada is roughly one-tenth as populous as the United States. This seemingly surprising conclusion follows from an assumption of constant returns to scale: cooperating with q states of equal size provides a state with q times the cooperative benefits of cooperating with one state. Similarly, cooperating with a state that is q times as large as another provides q times as many benefits. When such conditions apply, different-sized states gain equally from cooperation.

Constant returns to scale is a reasonable, though imperfect, assumption for analyzing international cooperation. Cooperative returns are surely increasing in some range, playing an important role in the formation of the nation–state itself, as well as in the development of any international division of labor in either economic or security affairs. Returns are probably decreasing at some later stage, at least for states that are large and well integrated into the international system. But for an important range of intermediate levels of international cooperation, constant returns to scale is a useful initial approximation.[22]

To demonstrate that constant returns imply an equal division of costs and benefits between states, consider different-sized states as composed of different integral numbers of equal units. These units are the accounting measures for the relevant attributes constituting the "size" of a state for international cooperation. Because units are identical, cooperation between any two of them entails identical costs (c) and benefits (b) for each. Constant returns to scale means that the costs and benefits of any dyadic interaction are independent of other cooperative interactions in which either of those two units is engaged. Thus, the total net benefit to any unit from cooperation equals $b - c$ times the number of cooperative dyads to which it belongs. The total collective benefit summed over a group of S units all cooperating with one another equals $b - c$ times twice the number of dyads in the population or $[(b - c)(S!)/(S - 2)!]$.

If S represents the number of units in the international system, then state sovereignty is a partition of the system into n mutually exclusive and exhaustive subsets $\{s_1, \ldots, s_n\}$ where s_i is the size of the ith state, individual states may be of different sizes, and $\Sigma s_i = S$. By expanding the measure of collective cooperative gains in terms of this sovereignty partition and rearranging terms, we obtain

$$(b - c)\frac{S!}{(S - 2)!} = (b - c)\sum_{\substack{i=1\\ s_i \geq 2}}^{n} \frac{s_i!}{(s_i - 1)!} + (b - c)\sum_{i=1}^{n} s_i \{\sum_{j \neq i} s_j\}.$$

The left-hand side represents total benefits from global cooperation, while the right-hand side terms partition these into benefits from domestic and international cooperation, respectively. The first term represents the sum of domestic cooperative benefits within each of n states (i.e., the aggregate cooperative benefits among the s_i units constituting a particular state).[23] This domestic cooperation is taken as unproblematic from an international perspective and will not be discussed here. The second term on the right-hand side represents the benefits of complete international cooperation. Each state i receives $s_i s_j(b - c)$ from cooperation with state j, while state j simultaneously receives $s_j s_i(b - c)$ from cooperating with state i. Thus, the assumption of constant returns to scale implies that the gains of international cooperation between two states are proportional to the product of their "sizes" and are equally divided between the two states. Although I use the specific form $s_i s_j(b - c)$ below, the conclusion concerning the equality of the gains is most important in the general argument.

An example is cooperation between Canada and the United States. On the one hand, the United States is 10 times the size of Canada, so each Canadian unit benefits from cooperation with 10 times as many U.S. units as does each U.S. unit with Canadian units. On the other hand, 10 times as many U.S. units benefit from cooperation. Since the aggregate national gain from bilateral cooperation is the sum of the gains of their respective units, it is straightforward to see that these sums are equal for the two states. The key assumption here is of constant returns to scale. The Canadian units represent the 101st–110th cooperative partners for the U.S. units, whereas the U.S. units represent the 11th–110th partners for Canadian units. If cooperation with the 11th partner provides more benefits than cooperation with the 101st partner (i.e., declining returns), then aggregate Canadian gains will exceed U.S. gains from cooperation.

Under a constant returns assumption, gains from cooperation are proportional to the size of the involved states and are shared equally between them. In *absolute* terms, states have the same interest in cooperation regardless of their respective sizes. Because the potential benefits of international versus domestic cooperation are proportionately greater for small states, asymmetric interdependence places them in a more vulnerable bargaining position. I do not address such considerations except to show why relative gains create incentives for larger states *not* to take advantage of this bargaining strength and, if anything, to offer sweeter deals to smaller states. In terms of *relative* position (as measured by absolute gaps between states), cooperation does not lead to relative gains.[24] But relative considerations do provide additional incentive to break agreements, since states seek not only to do well for themselves but also to do better than others.

The Limited Impact of Relative Gains in Multilateral Settings

Now we can examine relative gains maximization among a set of states $\{s_1, \ldots, s_n\}$, where s_i represents the size of state i. As I have shown, cooperation between two states leads to an equal payoff $s_i s_j(b - c)$ for each. This is an absolute payoff, however, and we want to consider states that worry about relative gains as well. Let the parameter r (with $0 \leq r \leq 1$) represent the weight states place on relative gains and $(1 - r)$ the weight of absolute gains in their overall evaluation of outcomes. When $r = 1$, we have the special case where states care exclusively about relative gains; when $r = 0$, they care only about absolute gains. Thus variations in r allows us to examine the impact of relative gains considerations on cooperation.[25]

We need to address the possibility that states are concerned with relative gains compared to specific other states. Certain states are more threatening for geopolitical reasons, including proximity or competition for key resources, or because of their seemingly aggressive character or ideological differences or a history of grievances between the two states. Or a state may be inherently more threatening simply because it has greater capabilities. Conversely, another state is typically less threatening when it is far away, has no reason to be hostile, or is small. To incorporate different evaluations of different states, let every state have a set of weights $\{w_{i1}, \ldots, w_{in}\}$, where $0 \leq w_{ij} \leq 1$ indicates the emphasis state i places on state j's performance in evaluating its relative gains position.

Assume that states distribute their relative gains concerns across other states so that $\Sigma w_{ij} = 1$. This restriction follows straightforwardly from a standard intuition of international balancing: it is at least as dangerous to suffer a unilateral loss in a world with fewer actors as in a world with more actors.[26] The reason is that more actors enhance the possibilities of protecting oneself through forming coalitions; and, generally, the less well united one's potential enemies, the safer one is. For example, the United States would be less adversely affected (or at least no more affected) by a drop in its power if the Soviet Union had first disintegrated into its separate republics than otherwise. This assumption has the substantive implication that states are not paranoid in the sense of magnifying relative gains losses versus other states without regard to their importance for overall power or security considerations. Conversely, states cannot be deluded by artificially inflating their relative gains performance through comparisons to strategically insignificant states.

Denoting state i's absolute payoff from international interactions as P_{ia}, its payoff incorporating relative gains concerns (defined as differences between its absolute payoff and the absolute payoff of other states) is

$$P_{ir} = (1 - r)P_{ia} + r \sum_{j=1}^{n} w_{ij} \; (P_{ia} - P_{ja}). \tag{1}$$

Index r reflects state i's degree of concern with relative gains. The first term on the right-hand side indicates the importance of its own absolute gains, and the second term its evaluation of relative gains versus every other state. This is the objective function that state i seeks to maximize.[27]

To investigate the impact of relative gains on cooperation, consider the circumstances under which state i would maintain a cooperative agreement with state k. Here cooperation involves a dichotomous choice to cooperate (by providing the other a benefit at a cost to oneself) or not-cooperate at each play of a repeated interaction. Repeated play situations are of primary interest because self-enforcing cooperation requires that the incentives provided by future cooperation be sufficient to maintain ongoing cooperation against current incentive to not-cooperate. For simplicity, states consider only two supergame strategies, *tit-for-tat* starting with cooperation or *never cooperate.* Third-party states are assumed to maintain their current behavior and do not react to the outcome between i and k. The possibility for cooperation then depends on whether the joint choice of tit-for-tat strategies is an equilibrium or whether states have an incentive to not-cooperate (never cooperate) when others cooperate (tit-for-tat).[28]

If both states choose tit-for-tat, then, with constant returns to scale, each receives $(b - c)\ s_i s_k$ on every play of the game. With a discount factor of ϕ the current value of this stream of absolute gains is $s_i s_k(b - c)/(1 - \phi)$. Incorporating relative gains considerations, the payoff to state i from a joint tit-for-tat strategy choice is

$$P_{ir} + (1 - r)\frac{b - c}{1 - \phi}\, s_i s_k + r\frac{b - c}{1 - \phi}\, s_i s_k \sum_{j \neq k} w_{ij}. \qquad (2)$$

The first term represents the value of the preexisting situation (as shown in equation 1), while the second term is the absolute gains value of cooperation with k. The third term represents the relative advantage achieved over other states through cooperation with k. Since i and k gain equally from cooperation, neither achieves any relative gain over the other through joint cooperation.[29]

The stability of cooperation depends on whether an individual state i does better by breaking the agreement for immediate gains at the expense of destroying the longer-run cooperative agreement. If state i defects, it receives a first-round payoff of $s_i s_k b$ from its free ride on k's unilateral cooperation. This ends bilateral cooperation, since k will reciprocate in the next round and no further benefits will be received from the interaction. Meanwhile, state k incurs a one-period loss of $s_i s_k c$ from unilateral cooperation. After this loss, state k will cease cooperation and realizes no further costs or benefits from the dyad. Again, these are all absolute gains payoffs, and state i's overall evaluation—including relative gains considerations—can be seen in the light of equation 1 to be

$$P_{ir} + (1 - r)\, s_i s_k b + r s_i s_k b \sum_{j \neq k} w_{ij} + r s_i s_k (b + c)\, w_{ik}. \qquad (3)$$

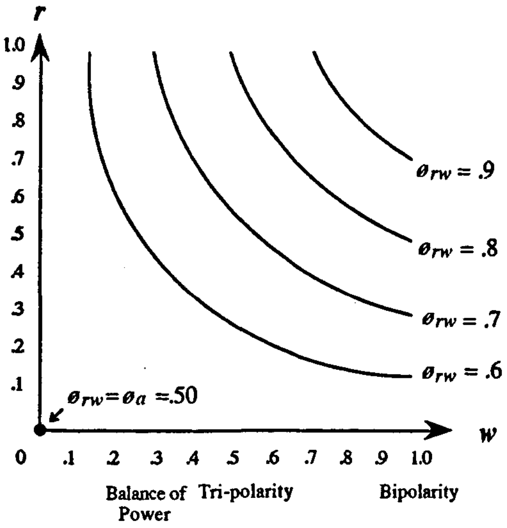
The first term is again the value of the preexisting situation, while the second term reflects the absolute gains from a free ride on state k's unilateral cooperation for one period. The third term represents the relative advantage achieved over third-party states through this free ride. The final term is i's relative gain over k, due not only to the value of i's free ride but also to the loss sustained by k through unilateral cooperation.

The stability of cooperation between i and k depends on whether the value of continuing cooperation in equation 2 exceeds the value of defection in equation 3. This will be the case if

$$\phi > \phi_{rw} = \frac{c + r w_{ik} b}{b + r w_{ik} c}, \qquad (4)$$

where ϕ_{rw} is the minimum discount factor that will support cooperation when state i places an overall weight of r on relative gains and a specific relative gains weight of w_{ik} on state k. Note that neither the absolute nor relative sizes of two interacting states directly affect their propensity to cooperate. Insofar as size affects relative gains weights (e.g., s_k partly determines w_{ik}), however, it indirectly makes

Figure 8. Isogram Curves for ϕ_{rw}



cooperation more difficult among larger states and dampens the willingness of smaller states to cooperate with larger states. I pursue possible implications of this below.

The key factors determining the impact of relative gains are r and w_{ik}. If either is zero, then equation 4 reduces to $\phi_{rw} = c/b$, the condition for cooperation when states care only about absolute gains (ϕ_a). That is, if state i either does not care about relative gains in general ($r = 0$) or its relative gains concerns do not involve state k in particular ($w_{ik} = 0$), then cooperation is not impeded.[30] At the other extreme, cooperation is impossible if r and w_{ik} are both unity since equation 4 reduces to the impermissible $\phi > \phi_{rw} = 1$. That is, if a state cares only about relative gains ($r = 1$) and evaluates them only with respect to one other state ($w_{ik} = 1$), there is no room for cooperation with that state. This is unsurprising, since it reduces the interaction to a two-actor pure relative gains situation that is strictly zero-sum, as shown earlier. Such a world is doubly unrealistic, since it assumes both that states do not care at all about absolute gains and that they inhabit a bipolar system so tight that no regard is paid to third parties.

Understanding the impact of relative gains in the multilateral setting requires analysis of the joint impact of r and w_{ik}. As in the two-actor case, the impact of increasing r is greatest at low levels, with positive but decreasing additional impact as r increases. The same is true for w_{ik} (i.e., $\partial\phi/\partial w > 0$ and $\partial^2\phi/\partial w^2 < 0$). Although this suggests that low levels of concern for relative gains have a significant impact on cooperation, as shown for the two-state case above, the interactive relation of these two factors decreases that impact dramatically. Figure 8 shows this relation for $\phi_a = .5$. The axes represent different levels of r and w_{ik} while the rectangular hyperboles are isograms for equal levels of ϕ_{rw} required to support cooperation. The origin is the point where there are no relative gains considerations so that $\phi_{rw} = \phi_a$. Finally, the horizontal axis indicates the cases of bipolarity, tripolarity, and classical balance of power involving two, three, and five states, respectively.[31]

Both r and w_{ik} must be large for relative gains to matter. Figure 8 shows that if either r or w_{ik} is small, then ϕ_{rw} is not substantially larger than ϕ_a. More importantly, unless both r and w_{ik} are large, the impact on ϕ_{rw} is at most moderate. Consider a tripolar system where relative and absolute gains are weighted equally, so that $r = w_{ik} = .5$. Relative gains considerations increase ϕ from an initial absolute gains $\phi_a = .5$ to $\phi_{rw} = .67$. This increase is substantially less than the extreme relative gains case, with ϕ increasing only one-third as much. The increase in ϕ is slightly greater for higher values of ϕ_a, but relative gains considerations still do not necessarily impede cooperation. Thus, the tripolar case already indicates a very different situation from the pure bipolar relative gains situation. Whenever there are more than three significant states

Table 2. Percentage Impact of Relative Gains under Different Combinations of r and w

	General Weighting of Relative Gains (r)									
	$\phi_a = .5$					$\phi_a = .8$				
System Type (n, w)	0	.25	.50	.75	1.0	0	.25	.50	.75	1.0
Bipolar ($n = 2$, $w = 1$)	0	33	60	82	100	0	38	64	84	100
Tripolar ($n = 3$, $w = .5$)	0	18	33	47	60	0	20	38	52	64
Balance of power ($n = 5$, $w = .25$)	0	10	18	26	33	0	11	20	29	38
10 countries ($b = 10$, $w = .11$)	0	4	8	12	16	0	5	10	14	18
20 countries ($n = 20$, $w = .05$)	0	2	4	6	8	0	2	5	7	9
Large n ($n \to \infty$, $w \to 0$)	0	0	0	0	0	0	0	0	0	0

or relative gains are weighted less heavily, the impact of relative gains is even smaller, as can be seen clearly in Figure 8.

Table 2 highlights this relationship numerically for this case and also for $\phi_a = .8$. The bipolar system with $r = 1$ represents the case where relative gains have 100% of their impact and fully inhibit cooperation. With large numbers of relevant actors or $r = 0$ (corresponding to the vertical and horizontal axes of Figure 8, respectively), relative gains have no impact (0%). Intermediate cases show the percentage of relative gains impact under the stipulated circumstances.

The impact of relative gains drops off rapidly whenever r and w are not *both* high. Again, the tripolar case is most dramatic. Adding a third actor to the pure bipolar relative gains world is equivalent to cutting the concern for relative gains in half (e.g., $n = 3$, $r = 1$ is equivalent to $n = 2$, $r = .5$) and reduces the impact of relative gains by about 40%. When both $n = 3$ and $r = .5$, the impact of relative gains is reduced by roughly two-thirds. Similarly, a move to a balance-of-power system (even with $r = 1$) is equivalent to a reduction of relative gains concerns by 75% and reduces their impact by two-thirds. In general, whenever $n > 2$ and $r < .5$, relative gains have no more than 40% of their potential impact, regardless of the initial value of ϕ_a. The drop-off is even greater for lower levels of r and w.[32]

Thus, the relative gains argument cannot provide a decisive response to the institutionalist claim that decentralized cooperation is possible under anarchy. Relative gains can save the realist case only in the two-actor world and perhaps demonstrate that institutionalists underestimate the difficulty of cooperation in other very-small-n cases. But these are much weaker claims than realists have made in uncritically transferring relative gains arguments from the two-actor world to international politics more generally.

Implications of Relative Gains for Contemporary International Relations Theory

Although the general claim that relative gains prevent international cooperation is not correct, some potentially interesting implications follow from the assumption. As I have shown, relative gains have their greatest impact when the number of states is small or (equivalently in terms of the model) there are asymmetries among them. I shall informally sketch out some plausible explanations that relative gains may provide for key aspects of the interaction between large and small states, hegemonic cooperation and decline, cooperation patterns in a bipolar system, and the possibility of increasing cooperation under multipolarity.

Why Do the Small Exploit the Large?

A common claim about international politics is that small states fare better than large states from their interactions. Examples include unequal burden sharing in military alliances (Olson and Zeckhauser 1966) and the need for some large country to absorb the costs of leading the international economy (Kindleberger 1973). The standard explanation for this putative phenomenon is that international cooperation is a public goods problem where large actors make the group privileged by unilaterally providing the collective good (Olson 1965). Difficulties with this account include such questions as whether the relevant international goods are truly public and why relative decline of the largest state does not always result in the collapse of public goods provision (Keohane 1984; Snidal 1985b). These objections are not necessarily fatal, but they require complicated supplementary arguments regarding the role of international institutions in promoting and maintaining cooperation.

The relative gains explanation is simpler and arguably more elegant. Although I did not assume it earlier, it is reasonable to believe that states are warier of larger states with regard to relative gains. Thus, for any pair of states, the smaller state will be more concerned with the relative gains consequences of their interaction (i.e., $w_{ij} > w_{ij}$ if $s_i < s_j$, ceteris paribus). The larger state may overcome the smaller state's greater reluctance to cooperate by offering it more than an equal share of the benefits. It is in the large state's interest to do so because it prefers an unequal cooperative arrangement to no cooperation. Therefore, an asymmetric distribution of absolute gains may be a requisite for striking cooperative agreements among different-sized states concerned about relative gains.

Hegemonic Cooperation and Decline

Cooperative arrangements favoring smaller states contribute to relative hegemonic decline. As the unequal distribution of benefits in favor of smaller states helps them catch up to the hegemonic actor, it also lowers the relative gains weight they place on the hegemonic actor. At the same time, declining relative preponderance increases the hegemonic state's concern for relative gains with other states, especially any rising challengers. The net result is increasing pressure from the largest actor to change the prevailing system to gain a greater share of cooperative benefits.[33]

This relative gains argument may better explain changes in economic relations among Western states than in security relations where the public goods nature of defense spending (especially deterrence) is more compelling. Shifting concern over foreign trade and investment is one indicator of perceived changes in relative advantage. Whereas in the 1950s and 1960s Europe, Canada, and the Third World complained of U.S. dominance in trade and investment, in the 1970s and 1980s it was the United States that worried about foreign—especially Japanese—relative advantage. The United States now actively seeks to alter international trading rules in its favor whereas in the early postwar period it accepted special exceptions for European and less-developed states.

Cooperation Patterns in a Loose Bipolar World

The hegemony argument overlooks the obvious fact that the postwar international system combined two superpowers with a handful of secondary powers and a multitude of small states. As a first approximation, consider the relative gains consequences in a system with two great powers and many small ones. The large states weight each other heavily on relative gains and weight the small states barely at all. The many small states weight the great powers heavily and each other not very much at all. (Again, I set aside geopolitical, historical, and other factors that affect this pattern.) Therefore, small states find it easier to cooperate with each other than with large states. Large states find it difficult to cooperate with each other, but their willingness to cooperate with smaller states is not impeded by relative gains. Because small states are reluctant to cooperate with them, the great powers may offer favorable terms to small states as noted above. Indeed, competition between them for cooperative relations with small states seems likely. The result is the relative decline of the superpowers and therefore increased possibilities for cooperation between them.

This approximation to bipolarity explains many stylized facts of the postwar period. The superpowers found it very difficult to cooperate with one another. They competed to establish good relations with small states, especially in the Third World, where spheres of influence were more fluid. Cooperation among smaller states was evidenced in the early successes of the European Community. The greatest anomaly is perhaps relations among Third World states, where a number of major cooperative initiatives, including the Andean Pact, the Association of Southeast Asian Nations (ASEAN), nonalignment or the Organization of African Unity met limited success. Only in the late 1960s did Third World cooperation emerge in forums such as the Law of the Sea and the United Nations Conference on Trade and Development (UNCTAD), and some of this was more rhetorical than real. Finally, if the relative gains hypothesis is correct, it helps explain how competition between the two superpowers led to their mutual decline and improved prospects for cooperation between them. (See Snidal 1991 for a related discussion of the instability of bipolarity under relative gains.)

Multipolarity Versus Bipolarity

Some realists argue that bipolarity is preferable to multipolarity because states care about relative gains. Waltz, for example, proposes that it is easier to keep track of relative capabilities in a bipolar world and that the resulting reduction in uncertainty increases stability (1979, 168). However, the preceding analysis shows why it is less necessary to keep track of relative gains in a multipolar world. Because rational state behavior is less affected by relative gains, cooperation is easier under multipolarity. Therefore, realists who premise their arguments on relative gains should prefer multipolarity over bipolarity.[34]

This contrasts sharply with standard absolute gains arguments that increasing the number of states limits cooperation. Increasing *n* impedes retaliation against noncooperators whenever states cannot perfectly discriminate their behavior with respect to particular other states. It therefore limits the possibilities for decentralized enforcement of cooperative agreements. These problems have been set aside in the current analysis by treating interactions as dyadic, although the broader *n*-actor environment is crucial to each state's behavior in the dyad. Limiting the ability of states to differentiate their behavior across dyads also makes relative gains cooperation more difficult under increasing *n*. When discrimination is possible, however, increasing *n* has the opposite impact of promoting cooperations in the relative gains world.

It is premature to conclude that recent trends toward multipolarity have facilitated international cooperation. To be sure, there has been a short-run blossoming of cooperative East–West relations;

and it is possible to point to recent successes in intra-West relations. However, this follows a period of intense rivalry between the superpowers and is accompanied by continuing tensions over the management of the Western economy. In brief, there is insufficient evidence to support the claim that multipolarity has increased cooperation. What we can say is that relative gains arguments do not support claims that the increasing importance of other actors will be unsettling for the international system.

Defensive Cooperation Versus Defensive Positionality

Grieco (1990) argues that the possibility of *some* states' seeking relative gains leads *all* states to forgo cooperative opportunities out of fear that others will take advantage of them. He labels this *defensive positionality.* But relative gains need not have this effect and instead can lead states to *defensive cooperation.* As we have seen, concern for relative gains in respect to some states is consistent with cooperation with other states. More surprisingly, cooperation with relative gains adversaries can be the best choice in a multilateral world, especially as the number of states increases. States that do not cooperate fall behind other relative gains maximizers that cooperate among themselves. This makes cooperation the best defense (as well as the best offense) when your rivals are cooperating in a multilateral relative gains world (Snidal 1991).

Conclusion

The realist argument that relative gains seeking greatly diminishes possibilities for international cooperation is not generalizable. It applies in the special case of tight bipolarity between states that care only about relative gains. Its truth diminishes rapidly if concerns for relative gains are less than total, *or* if the initial absolute gains game between states is not PD, *or* (especially) if the number of states increases to three or more. Since one or more of these conditions characterizes most international political phenomena, relative gains do not limit international cooperation in general.

Yet relative gains hold other interesting implications for international politics. I have not addressed the veracity of the assumption that states aspire to relative gains, only examined its consequences. The resulting conclusions connect relative gains to prevailing hypotheses about international relations theory and are plausible in light of postwar experience. Many of these implications should be unsurprising to both institutionalists and realists, since they correspond to prevailing academic folk wisdom. Others do not fit well with realist beliefs—for example, the claim that relative gains seeking makes multipolarity preferable. All of them are fairly casual and require tighter analytical specification and closer empirical analysis. But they point toward the sorts of testable implications that will ultimately be the proof or disproof of the relative gains hypothesis.

Finally, the analysis suggests a moral for international relations theorizing more generally. The realist argument is guided by an intuition that relative gains transform issues into highly conflictual—even zero-sum—situations where cooperation is not viable. The intuition is correct when there are just two states. The error lies in uncritically mistaking conclusions from a two-actor model for general claims about international politics with any number of actors. One thing we increasingly know about theoretical claims is that they are sensitive to particular changes and contexts. Only a more-careful working-through of the analytical argument will enable us to understand their consequences.

Notes

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1. Grieco (1990) stresses this as a distinctive aspect of relative gains. As I point out, distributional concerns neither differentiate realism from liberal institutionalism as markedly as he would wish nor place the relative gains argument on its strongest footing. Much of Grieco's empirical evidence for relative gains is contestable because he cannot distinguish it from absolute gains bargaining over the distribution of cooperative benefits. Finally, his differentiation of the two models is biased, since his relative gains model includes absolute gains considerations but not the other way around. For a further discussion of these issues, see Snidal 1991.

2. I develop a simple model based on constant returns to scale where gains from cooperation are equal even among different-sized states. The point is that even if this were not the "natural" result, it could be achieved by renegotiating the terms of cooperation.

3. Misperception or uncertainty among risk-averse actors could still explain the failure of such deals. Although references to uncertainty are common in the relative gains literature (e.g., see the implicit reference to the quotation from Waltz in the text), it is not systematically developed as part of the relative gains argument. Moreover, uncertainty and risk aversity play a similar role under absolute gains and thus are not a distinctive part of the relative gains argument. Finally, as Andrew Moravcsik (personal communication 28 June 1990) has reminded me, relative gains analysts who wish to incorporate the risk of cooperation must also be attentive to the risks of the status quo of noncooperation in anarchy. Risk and uncertainty may also promote cooperation and are part of the standard explanation for international regimes. These considerations would be even more important in the *n*-actor situation.

4. This second aspect of relative gains seeking effectively incorporates the first, since cooperative agreements are evaluated in terms of relative payoffs. Now, however, the relative gains problem cannot be resolved by changing the terms of the agreement, since relative gains are fundamental to states and not a property of particular cooperative agreements.

5. Joanne Gowa (personal communication 26 September 1989) states this case succinctly: "Security gains, whenever they enter utility functions, are always relative." Rather than argue the point, I investigate its implications.

6. Elsewhere, I argue that there is no reason to presume that all international issues are adequately represented by PD or any other single strategic structure (Snidal 1985a). But that argument assumes states care about absolute gains, whereas the present one demonstrates how relative gains provide a basis for arguing that PD is a sufficient description for international issues.

7. Labeling particular behaviors as *cooperation* or *defection* independent of their consequences runs the danger of persuasive definition. This is exemplified by the peculiar definition of *mutual cooperation* in the *deadlock* game as an outcome where both sides are worse off than they would have been through *mutual defection* (Downs and Rocke 1990; Oye 1985). Such problems are avoided here by the stipulation that $M > 0$. Note, however, that in the two-actor situation gains from cooperation diminish to zero as relative gains concerns increase to unity.

8. Here we are interested primarily in the ordinal symmetry (i.e., same order of payoffs) necessary to define a two-by-two game; subsequently, the symmetry condition will be extended to entail interval-level symmetry with the algebraic comparisons of payoffs inherent to relative gains. This evaluation of relative gains requires that states compare gains but does not require interstate comparisons of utility. Thus, payoffs could be "objective" factors such as gains from trade or increases in military strength. Grieco argues against symmetry as an assumption because it entails equal payoffs from mutual cooperation (1990, 42). I have argued that this can be readily achieved by adjusting the terms of cooperation and is not central to the relative gains argument. Concerns for relative gains do help determine the mutually acceptable cooperative agreements—although that role is indistinguishable from bargaining over the distribution of absolute gains.

9. Four of the six games excluded by the $F > U$ restriction are strictly harmonious in that each side has a dominant strategy to cooperate. The remaining two games are variants of the included assurance and coordination games. In addition, in all six of these games where $U > F$, relative gains reinforce absolute incentives to cooperate. Note 17 expands on this point. Finally, an alternate third assumption to restrict the number of games is that free-riding is preferred to mutual cooperation, $F > M$. This produces four two-by-two games. Three of these ($F > U > M > 0$, $F > M > U > 0$, and $F > M > 0 > U$) have $F > U$ and so are included here. The fourth, $U > F > M > 0$, offers a minor variation on the included coordination game with $F > U > M > 0$, where relative gains facilitate cooperation.

10. Defining cooperative behavior in this game is

difficult. One state cooperates by acceding to the other state's preferred equilibrium outcome. The other state sacrifices nothing. Cooperation is therefore not well defined at the individual level but is defined at the collective level in terms of avoiding noncoordinated outcomes. When relative gains matter, cooperation is effectively redefined as choosing the mutually preferred noncoordination point. For this reason, this game fits awkwardly at several stages in the analysis. I give more detailed discussion of the contrast between PD and coordination elsewhere (Snidal 1985a).

11. Treating relative gains as differences in absolute gains is standard in the formal literature. Verbal discussions are often ambiguous, sometimes also referring to relative gains in terms of ratios or proportions. (See the Waltz quotation above.) The alternative formulation is easily handled here by treating absolute payoffs as the impact of cooperation on growth rates of payoffs and relative gains as differential growth rates.

12. This formulation is equivalent to Grieco's (1990), where $r = k/(1 + k)$ and k is his weighting parameter.

13. In the intervening case where $U = M - F$, there is no transition zone between harmony and the PD. Finally, games that are symmetric in ordinal but not interval terms undergo a similar transition except that the two ordinal payoff structures are transformed at different rates. This leads to some additional and nonsymmetric intermediate games during the transition.

14. See Taylor 1987. The transformation of chicken reminds us of the peculiar consequences of relative gains: once $r > U/F$, players in chicken prefer mutual noncooperation (or "disaster") to unilateral cooperation.

15. Grieco incorrectly reports that increasing relative gains concerns can transform PD into deadlock (1990, 43). Examination of Grieco's equation 4 shows that both sides cannot simultaneously have deadlock preferences.

16. Taylor (1987) develops this result. With two minor exceptions, the PDs that emerge in other games also become progressively more intense with the increasing importance of relative gains. The temptation to defect from mutual cooperation increases with r, provided $M > U$, which is true for all games considered here except coordination. The sucker cost of unilateral cooperation similarly increases with r, provided $F > 0$, which is true for all the games here except assurance. The advantages of mutual cooperation over mutual defection always decrease with increasing r.

17. This logic confirms that the assumption $F > U$ is appropriate for examining how relative gains affect cooperation. When $U > F$, the preference ordering under relative gains ultimately becomes $U^* > M^* > 0 > F^*$. This harmonious deadlock game offers each side a dominant strategy leading to a Pareto-efficient equilibrium. Now, relative gains incentives reinforce cooperative incentives in the initial absolute gains game. Thus, to study how relative gains impede cooperation, it is reasonable to restrict analysis to cases where $F > U$. This means, moreover, that the present analysis is constructed to be favorable to the relative gains case.

18. Formally, this measure is $(\phi - \phi_a)/\phi_r - \phi_a)$, where ϕ_a and ϕ_r are the minimum discount rates required to support cooperation under pure absolute and pure relative gains, respectively.

19. These values are used in Figure 5 to locate the transition points between games. They are not used in Figure 4 because, with $U = M - F$, these two scenarios collapse into one, with no transition zone between harmony and PD.

20. This is the case for other games, except chicken, at their point of transformation to PD. Chicken poses its own cooperation problem that can sometimes be resolved if states' concern for future benefits is sufficiently high. Coordination and harmony (when $U > M - F$) pass through an intermediate chicken phase, where the discount factor matters even before PD is reached at higher r.

21. The discussion in this paragraph is based on a "vertical" reading of Figure 7 to see the value of ϕ indicated by a given level of r. An alternative "horizontal" reading shows the level of r necessary to make a given ϕ the lowest discount factor amenable to cooperation. This allows a comparison of the different levels of relative gains necessary to create a PD as intense as the standard PD when $r = 0$ and $\phi = .5$. Chicken requires $r = .33$; assurance I requires $r = .6$; assurance II requires $r = .75$; and harmony requires $r = .8$. Coordination always requires a higher ϕ, regardless of r, reflecting the peculiarities it raises for the concept of cooperation as discussed above. These results strongly confirm the limited impact of relative gains in creating intense PDs from other absolute gains situations.

22. A variety of returns-to-scale assumptions appear in the literature. In economics, the standard trade model assumes constant returns, whereas analyses of the international division of labor and the "new" theory of international trade consider increasing returns to scale. On the security side, returns will be increasing with respect to alliance membership for public goods aspects of security provision (Olson and Zeckhauser 1966). Friedman (1977) explains the size and shape of nations by increasing returns to tax collection on trade and labor with constant returns to taxation on expanding territory.

23. The restriction $s_i \geq 2$ reflects the assumption that at least two units are required for cooperation as defined here. If $s_i = 1$, there is no domestic cooperation as measured in terms of these units. The measure of domestic cooperative benefits depends

on the definition of units because some cooperative activity that occurs *within* larger units is external to (i.e., occurs *between*) the smaller units. The measure of international cooperation is not affected by changes in units.

24. The situation is different if we consider gains from purely domestic cooperation. Under constant returns and autarky, larger states grow by absolutely larger amounts and therefore achieve relative gains. The situation is also different if we define relative gains in terms of the ratio of sizes. With constant returns and full global cooperation—that is, all domestic and international cooperative interactions occur—all states grow at the same rate and there are no relative gains. Because small states depend on international cooperation for a larger portion of their growth, large states could achieve relative gains by breaking off international cooperation. This ratio measure of relative position is not used here; but it is straightforward to see that increasing numbers of states decrease these incentives for noncooperative behavior for similar reasons as when states care about relative gains defined as differences.

25. Because we assume a PD situation, note that "r" is overstated if the absolute gains is not PD. Assurance I, for example, only becomes PD when $r > .5$ and the impact of relative gains on it is less than for PD until $r = 1$. Thus, the results are biased toward the relative gains hypothesis for the other five absolute gains games.

26. The key part of the restriction is $\Sigma w_i \leq 1$, since higher sums favor the relative gains position. This follows directly from the balancing intuition for the symmetric case. If a state suffers an absolute gains loss of π in a world with n other states, each weighted w_n, its relative gains payoff decreases by $(1 - r)\pi + r\pi n w_n$, as will be seen in equation 1. Since this is preferable to suffering the same absolute loss in a world of $n - 1$ other states, each weighted w_{n-1}, it is straightforward to show $w_n \leq ((n - 1)/n)w_{n-1}$. Because $w_1 = r \leq 1$, $w_2 \leq 1/2$. It follows by forward induction that $w_3 \leq 1/3$ and, in general, that $w_n \leq 1/n$. Therefore, it must be that $\Sigma w_i \leq 1$. A parallel proof leads to the same conclusion regarding the sum of weightings when gains in relative power are more valuable in a world with fewer states.

27. We ignore payoffs from domestic cooperation, since these are not affected by international cooperation under constant returns. Note that if $r = 1$ and all states weight all other states equally in relative gains (i.e., $w_{ij} = w_{kl}$ for all i, j, k, l) then $\Sigma P_{ir} = 0$ (Snidal 1991). Unequal w's allow the sum of relative gains in the system to be positive or negative.

28. This comparison establishes the necessary conditions for a stable cooperative equilibrium. If joint tit-for-tat strategies are stable, there will be a multitude of other cooperative equilibria. Showing the minimum requirements for the existence of one such equilibrium is sufficient for present purposes of investigating how much more difficult cooperation is under relative gains. For a more detailed exposition, see Snidal 1991.

29. Unequal gains diminish the incentives of one side to cooperate (and increase the incentives of the other side) for both absolute and relative gains reasons. I have discussed how this aspect of relative gains can be handled by changing the terms of bilateral cooperation—notwithstanding the more important point that the two states still evaluate outcomes in terms of relative gains. Later I shall discuss circumstances under which an asymmetric distribution of benefits actually improves prospects for cooperation.

30. This parallels the finding cited above that relative gains have no impact in symmetric large-n situations, since large n implies small w_{ik} for all i,k. For a more detailed discussion with special attention to tipping points and the stability of the cooperative equilibrium, see Snidal 1991.

31. This assumes that other states are weighted equally in terms of relative gains (i.e., $w_{ik} = w_{ij}$, for all j,k) so $w_{ij} = 1/(n - 1)$. That is unproblematic for bipolarity, where there is only one other state, and for balance of power defined as a rough equality of the great powers. A perfect tripolar system would also have this property, although it is easy to imagine one state placing different weights on the other two major powers. Such a "two-and-a-half-power" system would fall between tripolarity and bipolarity as depicted here.

32. The maximum impact of r in percentage terms occurs when ϕ_a approaches unity and equals $(2rw/rw + 1)$ (100%). An example of the continuing drop-off of relative gains effects is the five-actor balance-of-power system ($w = .25$) with $r = .25$ where the impact of relative gains must be less than 12%. The increase in n is defined in terms of significant actors, as I have discussed. A tripolar system, for example, has three significant actors regardless of the number of minor actors. These pure cases never exist but provide appropriate benchmarks for my arguments.

33. The relation between size and relative gains weights may be more complicated than the monotonic relation used here. A plausible curvilinear specification is that relative gains concerns peak when states are roughly equal and drop off when one state is either far behind, or far ahead of, the other. This would seem particularly appropriate for analyses of interactions between a rising challenger and a declining hegemon. It would alter the explanation for the posthegemonic breakdown of cooperation from that in the text to emphasize rising tensions between these two central actors. Finally, relative gains concerns may vary across issue dimensions. It makes little sense for small states such as Canada and Finland to worry about relative gains in security against their militarily formidable neigh-

bors. But they may worry about other dimensions such as economic relations where they are better able to counteract relative gains.

34. This supports Kaplan's view of multipolarity as fostering a complementarity between states (1957, 23). (Kaplan assumes that states measure relative gains against the system, not against each other.) Grieco argues that under relative gains, cooperative deals are easier when they involve larger numbers of states (1988a, 506). I show that what is important is the number of states that affect relative gains calculations, not how many participate in any particular cooperative deal.

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NUCLEAR DETERRENCE, COUNTERFORCE STRATEGIES, AND THE INCENTIVE TO STRIKE FIRST

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I use the theory of games to investigate issues about how to understand the use of nuclear counterforce strategies by the United States and the Soviet Union during the Cold War. The function of the counterforce strategies I model is not to enable a state confidently to launch a nuclear attack but to convince its adversary that the probability that it might do so as a last resort is greater than zero. The models allow one to investigate rational behavior when information is incomplete and there is an incentive to strike first, and therefore provide a way to explore controversies about the effect of counterforce strategies on both the credibility of extended deterrence and the possibility of inadvertent nuclear war. The models suggest, contrary to the claims of a number of writers, that the use of nuclear counterforce strategies is not necessarily inconsistent with rational behavior and provide some insight into the relation between counterforce strategies and brinkmanship models of deterrence.

Before the nuclear era, most military strategy focused on how to use a state's military forces to destroy the military forces of the enemy. With the development first of strategic bombing and then of nuclear weapons and ballistic missiles, however, strategists had to take into account the fact that a state could damage an enemy's population centers and destroy things of value to him without first destroying his military forces. And when both the United States and the Soviet Union acquired secure second-strike capabilities, the use of strategic nuclear weapons as a means of destroying the enemy's military forces seemed to many to have become pointless; for, by definition, neither side could then destroy enough of the other's strategic nuclear forces to reduce to acceptable levels the damage the enemy could do in retaliation.

The existence of secure second-strike capabilities also seemed to imply that the U.S. could no longer credibly threaten to respond to a Warsaw Pact invasion of Western Europe with a nuclear attack on the Soviet Union. Rather than simply accept this conclusion, however, much ingenuity was devoted to the problem of making the threat of such an attack sufficiently credible for it to continue to serve as a deterrent. Counterforce strategies (i.e., strategies for using nuclear weapons to destroy the enemy's nuclear forces) played an important role in discussions of how to make the threat credible. But since no feasible counterforce attack could prevent the United States from suffering an unacceptable level of damage if the Soviet Union retaliated, these suggestions met with a great deal of skepticism (Eden and Miller 1989; Freedman 1981, 123–38, 225–72, and 372–95). Although deterring a Soviet attack on Western Europe is no longer the main focus of United States defense policy, this controversy is still important, both for our understanding of military strategy during the Cold War and

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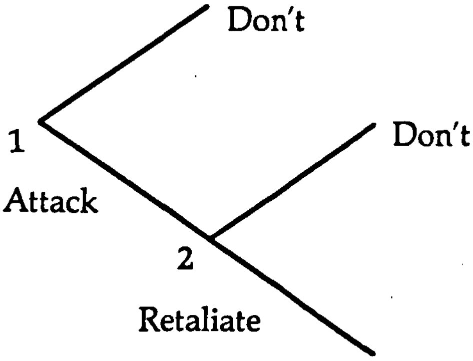
Figure 1. Assured Destruction



for our understanding of how nuclear weapons might be used in the future.

The basic argument for the utility of counterforce strategies can be stated very simply. The concept of mutual assured destruction implies that no matter which adversary strikes first and how powerful its attack, its opponent can absorb the blow and still do an unacceptable amount of damage in retaliation. Such a situation is represented in Figure 1. Since the state attacking first anticipates a devastating response from the victim, many would argue, it will not attack and its threat to do so will not be credible.[1]

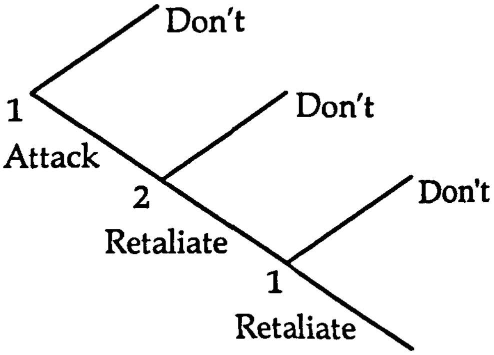
Note, however, that two assumptions are required for such a conclusion: that the victim will have the capability to retaliate and that it will choose to do so. One justification for assuming the victim will choose to retaliate is the belief that after the enemy's attack, it will have nothing left to lose and thus no reason not to retaliate. But counterforce strategies can create a situation in which the enemy still has a lot left to lose, even though it has suffered a great deal of damage.

Such a possibility is represented in Figure 2, where the attacker's first strike has been directed away from the victim's cities and against his strategic nuclear forces, leaving the victim with only enough forces to deliver the sort of countervalue retaliatory attack assumed in the assured destruction scenario. If the victim now retaliates against the attacker's cities, the attacker will still have enough forces left over to destroy the victim's cities in turn. By the same logic as that of mutual assured destruction, the victim will be deterred from retaliating by the prospect of the *attacker's* second strike; thus, the attacker can credibly threaten to launch a first strike against the victim. This scenario was implicit in the no-cities doctrine introduced during the Kennedy administration and has played an important role in debates about nuclear strategy ever since. For example, it was one basis for worries about the vulnerability of U.S. land-based missiles to a Soviet counterforce attack, which dominated debates about nuclear strategy in the United States beginning with the Carter administration.[2]

But this simple illustration implies no more than that counterforce strategies cannot be rejected out of hand as meaningless or irrelevant in a world of secure second-strike capabilities. It does not establish their feasibility, much less their desirability. There are many reasons for doubting both.

Some of the reasons that are given about nuclear strategy are less conclusive than they appear to be, however. For

Figure 2. Three-Strike Sequence



example, it is sometimes argued that no one could be confident enough of the effects of a counterforce strike to be willing to launch such an attack, as long as the victim had a secure second-strike capability. But since the point of a counterforce strategy is not to enable one confidently to launch nuclear attacks but to deter one's adversary, this criticism is not decisive. The real question is whether having counterforce capabilities increases the credibility of one's threat to launch such an attack in the mind of the adversary.[3] If Soviet leaders believed that U.S. leaders had confidence in their counterforce capabilities, a U.S. nuclear attack may have seemed sufficiently likely that the Soviets would be deterred from taking military action against NATO. And in the case of a Soviet attack against U.S. land-based missiles, the issue was not whether the Soviets could confidently launch a nuclear attack on the United States secure in the belief that the United States would not retaliate but whether the Soviets or the United States' European allies might believe that a U.S. president would be intimidated by that possibility in a crisis.

Thus, in considering the utility of counterforce strategies, it is not helpful to assume either that everyone is confident that the situation is as depicted in Figure 2 or that everyone is confident that it is not. The point of such strategies is to exploit the fact that no one can be sure what the situation is, or what the other actor believes it is.[4] The role of counterforce strategies in the nuclear era can therefore only be comprehended by explicitly introducing uncertainty into the analysis.

But counterforce capabilities would have to be used before they are destroyed by the enemy's own counterforce attack; therefore, they imply an incentive to strike first. Thus, if both sides employ such strategies and in the course of a crisis one succeeds in convincing the other that it is likely to be willing to resort to a nuclear attack to protect its interests, the adversary, instead of concluding that it should back down, may decide to launch a preemptive counterforce attack of its own. This is another argument that is often used against counterforce capabilities.

In two important recent papers on deterrence theory, Robert Powell (1989a, 1989b) has discussed both counterforce strategies and the problem of first-strike incentives, though not in the same context. In a paper on the strategy of limited retaliation, Powell examines the idea that states can use limited nuclear attacks to make the threat of future punishment more credible (Powell 1989b). He defines a game in which states can choose between massive nuclear retaliation and a series of limited nuclear exchanges. In these circumstances, only a state with nothing left to lose can credibly threaten to destroy its adversary. Thus, each state would want to avoid leaving its adversary with nothing left to lose, so that both would prefer limited to massive forms of retaliation.

Powell points out that counterforce capabilities can be used to put an adversary into a position in which it must choose between an unlimited attack and submission. Since an unlimited retaliatory attack by the victim of a counterforce attack would leave the initiator with nothing left to lose, the victim would choose instead to submit (1989b, 513). Counterforce attacks, however, are not an option in the game Powell analyzes, which assumes that states must choose between massive nuclear strikes and limited attacks that do a fixed amount of damage. Moreover, Powell assumes that it is common knowledge which state, after a sequence of limited attacks, would be the first with nothing left to lose. Only this state could credibly threaten to resort to an all-out nuclear attack; thus, its adversary would in the end have to back down. The only uncertainty concerns whether the player disadvantaged by this situation will behave rationally. Given such uncertainty, however, a disadvantaged player can feign irrationality by refusing initially to submit, in which case there is a chance that his adversary will choose to submit instead.

But any advantage that one state has in such a sequence of limited attacks can be trumped, as Powell himself suggests, by the counterforce capability of the other. Moreover, a counterforce attack may itself entail sufficient destruction to be an adequate deterrent if credible; and if a state believes it has an adequate counterforce capability and is sufficiently desperate, a willingness to use it need not be inconsistent with rationality, as the discussion of Figure 2 illustrated. Thus, Powell's model does not capture the idea that counterforce capabilities can increase the credibility of extended nuclear deterrence. Nor does it describe a situation in which there may be an incentive for preemptive attack.

In another paper, on the problem of crisis stability, Powell points out that counterforce capabilities "are criticized as being destabilizing" because they "contribute to the creation of first-strike advantages" (Powell 1989a, 62). This paper, however, is devoted to the analysis of a situation in which both states prefer not to attack at all to attacking first and would choose to attack only if that were the only way of avoiding being the victim of a first strike by the other side. Thus, the only reason either side would attack is that it believes that if it does not attack, the other side will. Powell shows that in a large class of situations with this property, no attack will occur, even though both sides have an incentive to attack first.

This is an important conclusion, since many people have believed that the mere existence of first-strike advantages can be a cause of war, even though neither side has any real interest in attacking the other. But Powell's assumptions do not capture the incentives of states in a situation in which counterforce strategies have been introduced as a means of making extended deterrence more credible. For the point of such strategies is precisely to foster the belief that there are circumstances in which one would attack even if one did not fear that the enemy was about to attack, namely, if one was on the verge of defeat in a conventional war. Thus, while it may in fact be true that both states prefer no attack at all to any sort of attack, the point of having counterforce capabilities is to convince one's adversary otherwise. It therefore cannot be common knowledge that this is true.

Powell establishes four conditions that are sufficient to insure stability:

1. A nuclear attack can only occur if some state intentionally orders it.
2. Whenever a state has the option of attacking, it also has the option of trying to submit to its adversary; and if its adversary has not already committed itself to an attack, this attempt to submit will succeed.
3. No state will attack unless it believes that the probability that an attack by the other is inevitable is greater than one-half.
4. It is common knowledge that the first three conditions are satisfied.

The existence of counterforce strategies seems to imply that the fourth condition cannot be satisfied, since if states have used counterforce strategies to increase the credibility of their threats to launch nuclear attacks, it cannot be common knowledge that the third condition is satisfied. This fact suggests that the relation between counterforce strategies and crisis stability needs to be reexamined. To do that we will need to construct a model in which a nuclear counterforce attack is one of the options available to states.[5]

Deterrence and Counterforce Strategies

It seems clear from the preceding discussion that any model designed to investigate the effects of nuclear counterforce strategies should have three features:

1. Neither actor should be certain of either the other's willingness to launch an initial counterforce attack, or of the other's unwillingness to do so.
2. A state should consider launching such an attack only to avoid a major loss, such as would result from the loss of a large-scale conventional war.
3. If a state is willing to launch an attack in such circumstances and believes the other is willing to do so as well, it should want to launch its own attack first.

While these features imply a considerable amount of uncertainty, it seems reasonable that they should themselves be common knowledge. In addition, the point of counterforce strategies is to deter the sort of conventional war that might provoke their use; and one of their disadvantages is that threatening a counterforce attack might instead provoke a preemptive attack by the enemy. Thus, to understand their effects, we must allow the adversaries in a conflict at least three options: to engage in a conventional war, to launch a nuclear counterforce attack, or to do neither. Moreover, once a conventional war has begun, choice among these options must be made repeatedly, until one side or the other has lost its ability to continue the war. Beyond this, there is room for disagreement about what assumptions one should make. One important question is whether each actor, when deciding whether to launch a counterforce attack or not, can be expected to know whether the other has already initiated such an attack.

The game Powell analyzes in his paper on crisis stability begins with a chance move that determines which player will move first (Powell 1989a). Thus, each player, in deciding whether to attack, is uncertain whether the other has already made his choice. At first glance, this may appear to be a way of representing the sort of problem emphasized by Posen (1982), who argued in a frequently cited paper on inadvertent nuclear war that when strategic nuclear capabilities are located close to conventional military forces, they may be threatened by conventional military attacks. Thus, conventional military operations might be interpreted as part of a strategic counterforce attack, and the target of such operations may be uncertain whether its adversary has already decided on such an attack or not.

In Powell's game, however, the actors choose merely between attacking and not attacking; they do not have the additional option of waging a conventional war. If this additional choice is added, then one must distinguish between two different questions: If an actor postpones its counterforce attack but continues conventional military operations, will it suffer a degradation in its strategic counterforce capabilities? and If an actor decides to stop fighting entirely, can it always do so in time to avoid its adversary's nuclear counterforce attack? Somewhat surprisingly, it is the latter question that is important, and not the former.

It is not clear that there is a uniformly best assumption to make about the answer to this question. It may be impossible in the heat of battle to reliably communicate to one's enemy that one wants to stop fighting before he carries out any plans he has made to launch a preemptive counterforce attack. On the other hand, this need not be impossible. Which assumption is the correct one? We will see that this is a crucial question that has been little discussed, and that an evaluation of much of the conventional wisdom on this subject depends on what sort of answer one gives to it. I will initially assume that a state can always be certain of avoiding a nuclear war by quitting the conventional conflict.

A Game-theoretic Model

In defining the rules of a game that are consistent with the assumptions made so far, it will be helpful to bear in mind the context in which counterforce strategies have mainly been discussed in the United States, that is, as a way of deterring a Warsaw Pact invasion of Western Europe. If Soviet leaders believed that it was likely enough that the United States would respond to the prospect of defeat in a conventional war in Europe with a nuclear counterforce attack against the Soviet Union but were unwilling to launch a preemptive nuclear attack on the United States, they would not invade Western Europe even if they had a strong incentive to do so. On the other hand, if Soviet leaders believed that U.S. leaders would not launch such an attack and feared provoking a preemptive Soviet counterforce attack on the United States, then Soviet leaders might expect the United States not even to resist with conventional weapons. If they were wrong in this belief, then a conventional war in Europe could occur; and to evaluate the utility of counterforce strategies we must consider what would happen in that case—in particular how likely it is that the result would be an inadvertent nuclear war.

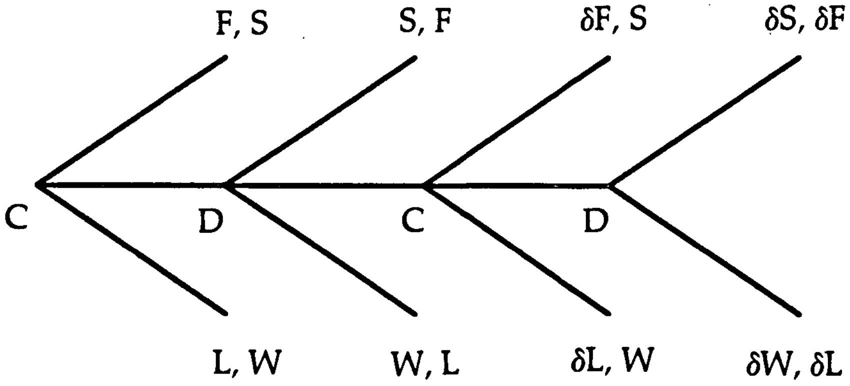
These scenarios suggest a game in which the Soviets have the first move, at which they must decide between a conventional invasion of Western Europe, a nuclear counterforce attack on the United States, and doing nothing. If they decide on an invasion, then the United States must choose between resisting the invasion with conventional weapons, launching a counterforce attack on the Soviet Union, and nonresistance. If the United States decides to resist with conventional weapons, the Soviets must decide whether to continue the conventional war, launch a counterforce attack on the United States, or withdraw—and so forth.

A conventional war can be ended at any move either by quitting or by launching a nuclear counterforce attack on one's adversary. If neither state ever selects either option, however, then the conventional war will continue. It could lead to many possible outcomes, about which there might be great uncertainty. Since counterforce strategies have been discussed primarily as a way of compensating for the conventional military superiority of the Warsaw pact during the Cold War, I will assume that it is common knowledge that the Warsaw Pact would win a conventional war that was fought to its conclusion. This assumption implies that the United States owns the last move in this game, since if it does not launch a nuclear counterforce attack, the Warsaw Pact will be victorious.

The number of moves one should allow between the first and last is, to some extent, an arbitrary question. In order to explore both sides' incentives to launch a preemptive attack in the course of a conventional war, the Soviets must be allowed at least one move prior to the final move of the United States, and the United States must be allowed at least one move prior to the Soviets' last one. This implies a game with at least four moves, such as the one represented in Figure 3, where the Soviet Union is labeled *C* (challenger) and the United States is labeled *D* (defender). The horizontal line in the figure consists of a sequence of decisions to initiate or continue a conventional war. The branches above the horizontal line represent nuclear counterforce attacks, while branches below the line stand for decisions to quit the conflict.

In this game the moves following a nuclear counterforce attack have been

Figure 3. Conventional War with Counterforce Threats



omitted for the sake of simplicity. As we have seen, counterforce attacks can lead to a situation in which the victim has a strong incentive not to retaliate, even though he still has the capability of doing so; but no attacker could ever be certain that a counterforce attack would have such an effect. The effect of counterforce attacks could therefore be modeled by assuming that after an initial counterforce attack, chance determines whether, in any subsequent series of attacks, the victim has the last move (as in Figure 1) or the attacker has the last move (as in Figure 2). In the former case, the victim will retaliate, and in the latter case the victim will not retaliate.

A strategically equivalent but much simpler representation, however, is to assume instead that a counterforce attack leads to a lottery, which gives to the attacker the value associated with prevailing over his enemy with a probability p (if the victim does not retaliate), and the value associated with a countervalue retaliatory attack with a probability of $(1 - p)$ (if the victim does retaliate). In the following analysis, therefore, a nuclear counterforce attack will be treated as though it led to a terminal node, and the payoff to the attacker at that node will be treated as a lottery. The expected value of a nuclear counterforce attack for the state choosing that option is labeled F (first strike), while the value of such an attack for its victim is labeled S (second strike).

Note that S incorporates the value that the victim places on being subjected to a *counterforce* attack, while F is a function of the probability $(1 - p)$ that the attacker will receive a *countervalue* retaliatory attack. There are two bases for the idea that states may have an incentive to strike first. One is that by striking first the damage that will be done by the enemy's expected attack will be reduced; the other is that by striking first one can reduce the enemy's incentive to attack. Powell (1989a), in effect, focuses entirely on the damage-reducing effects of a first strike, while the focus here is on its incentive-reducing effects. Nonetheless, I will follow the assumption in the literature on damage reduction that it is better to suffer the enemy's retaliatory strike than to allow him to attack first. I will call the value the attacker places on suffering the enemy's possible retaliatory strike R (retaliation).

If a player chooses to quit (or not to challenge), then the payoff is L (lose), and the opponent's payoff is W (win). These are also the payoffs if the conventional war is fought to a conclusion, in which case I assume that the challenger will win and the defender will lose.

Getting one's way by fighting a war, however, is obviously not the same as prevailing because one's adversary decides not to attack (in the case of the United States) or resist (in the case of the USSR). Thus, I assume that the value of both winning and losing is discounted by a constant factor δ $(0 < \delta < 1)$ from one move to the next; δ represents the effect of all the costs associated with fighting a conventional war. Thus a player that faces certain loss at some time $t + n$ will prefer to quit at time t. I assume, however, that both players would prefer winning a conventional war to the consequences of giving up without fighting.

I also assume that the expected values of first and second strikes are progressively reduced by the same factor δ. Note that it does not matter whether this is simply the result of the normal costs of fighting a conventional war or the fact that conventional military operations will degrade one's strategic nuclear capabilities and therefore reduce the expected value associated with being either the initiator of a counterforce attack or its victim. Thus, δ incorporates the effects of conventional attacks on nuclear capabilities emphasized by Posen (1982).

I assume that for both sides, W is the most preferred outcome. This implies that it is common knowledge that if Soviet leaders were certain the United States would not launch a nuclear counterforce attack in response, they would want to initiate a conventional attack against NATO. This assumption is made entirely for convenience. Obviously, there would be no point to nuclear deterrence if the probability that the Soviets would ever want to initiate such an attack were zero; it would not alter the analysis, however, if this probability were less than one or even close to zero. I also assume that both sides prefer first strikes to second strikes and that they both would prefer to lose than to be the victim of a first strike.

For the sake of simplicity, I will also assume that W is, for both players, equivalent to "winning" by launching a nuclear counterforce attack without suffering a retaliatory response. Thus $F = pW + (1 - p)R$. All that is really important for the following discussion, however, is that neither state prefers winning by a nuclear attack to winning by the other's conceding defeat at the outset; thus, neither would launch such an attack if it were certain that the enemy would not oppose it with conventional weapons in Europe.

I assume that all these features of the game are common knowledge. Thus, for each state the only uncertainty concerns whether one's opponent prefers a first strike to losing or has the opposite preference. In the game in Figure 3, therefore, each state could be characterized by one of two sets of preferences: either $W > L > F > R > S$, or $W > F > L > R > S$. States with the former preferences will be called *bluffers*, and states with the latter preferences *nonbluffers*. The probability that state i is a bluffer will be called b_i. Since W and S are the best and worst outcomes for both players, they can be normalized to one and zero, respectively. Thus $F = p + (1 - p)R$. This notation, along with additional notation that will be developed later, is summarized in Table 1.

United States Superiority

In this model the parameter p is a function of the military technology available to both sides. It obviously need not be the same for both players. Thus, it provides a way of investigating the significance of

Table 1. Notation and Definitions

Notation	Definition
C	challenger
D	defender
W	utility of winning ($W = 1$)
L	utility of losing
S	second strike—utility of being subjected to other side's first-strike counterforce attack ($S = 0$)
R	retaliation—utility of being subjected to other side's countervalue retaliation for one's own first-strike counterforce attack
p_i	probability that a first-strike counterforce attack by i will leave the victim with an incentive not to retaliate
F	first strike—expected value of first-strike counterforce attack ($F = pW + [1 - p]R$
δ	factor by which payoffs are discounted from one choice node to the next
bluffer	actor for whom $L > F$
nonbluffer	actor for whom $F > L$
L_{crit}	value of L such that $L_i > L_{crit} \rightarrow L_i > F_i$
$G_i(x)$	probability that $L_i \leq x$
b_i	probability that i is bluffing, i.e., that $L_i > F_i$ (thus, $b_i = 1 - G_i[L_{crit}]$)
b_i^*	updated value of b_i, given i's behavior at previous choice node
C_i, D_i	challenger's (defender's) ith choice node
$e(C_i)$, $e(D_i)$	expected value to challenger (defender) of choosing conventional war at ith choice node

the balance of strategic forces once both sides have secure second-strike capabilities. During the Kennedy administration, one might argue, if there was a significant sense in which the United States could be said to have had nuclear superiority over the Soviet Union, its superiority consisted of the fact that it had a minimally credible nuclear counterforce capability and the Soviet Union did not. Analysis of a situation in which only one state has a counterforce capability may therefore illuminate strategic issues that arose during this period. It is also a simpler problem than the two-sided case, and an understanding of it will provide a useful introduction to the more complex relationship that results from nuclear parity.

Let us therefore begin by assuming that it is common knowledge that the sequence of moves that would follow a first strike by the Soviet Union is represented by the game in Figure 1, and thus that $p_C = 0$. It follows that it is common knowledge that Soviet leaders have the first type of preferences. Let us also assume that prior to play of the game, U.S. leaders have taken steps to convince Soviet leaders that they would initiate a nuclear counterforce attack if that were the only alternative to losing. At the outset, therefore, the Soviets assign some probability greater than zero but less than one (perhaps significantly less than one) to the possibility that U.S. leaders have the second set of preferences.

In game theory, one represents uncertainty about the players' preferences by assuming that prior to the play of the game, a chance move selects the actual players in the game from a set of possible players, each of whom has one of the types of preferences that are assigned positive probability. Each player knows its own type but knows only the probability with which each of the possible opponents is selected. These probabilities are assumed to be common knowledge.

In this game there are only two types of preferences; associated with each type, however, are infinitely many possible sets of cardinal utilities. It will be convenient to begin by assuming that there is only one possible representative of each of the two types of preferences, so that each player can assign only one of two sets of cardinal utilities to the outcomes. This is not very realistic and will later be replaced by an assumption that is both more general and more plausible. Prior to play of the game, then, it is common knowledge that the challenger has the first type of preferences and that the challenger assigns the probability *b* to the possibility that the defender is bluffing in his threat to resort to a nuclear counterforce attack.[6] The defender knows, of course, its own type of preferences.

With these assumptions it is obvious that the challenger will never choose a nuclear counterforce attack; for whenever the challenger has the opportunity to launch a nuclear counterforce attack it also has the opportunity to quit the game and prefers the consequences of quitting. If, on the other hand, the challenger believes that the probability of a U.S. nuclear attack is small enough it will initiate a conventional military attack.

Suppose, then, that it does so. The defender, if not bluffing, will certainly not quit. It must choose, then, between immediately launching a nuclear counterforce attack and temporarily fighting a conventional war in expectation of losing. The only reason for postponing the attack is that the challenger may choose to quit at his next move. But this will happen only if the challenger's estimate of *b* (the probability that the defender is bluffing) is lowered, since if it were common knowledge that the challenger expected at the outset to quit, it could not be an equilibrium for the challenger to attack at his first move.

It has often been suggested that by initially resisting a Soviet attack with conventional weapons, the United States could increase the credibility of its threat to launch a nuclear attack against the Soviet Union. Let us see whether there is any basis for this suggestion in the context of this model.

The challenger will lower his estimate of *b* only if, in equilibrium, the probability that a bluffing defender will choose to fight a conventional war is less than the probability that a nonbluffer will do so. But if, in equilibrium, there were no probability that a bluffer would fight, continuation would lead the challenger to conclude that the probability that the defender was bluffing was zero; and a bluffer would therefore want to fight after all. Thus, if there is any probability that a nonbluffer will fight, there must also be some probability that a bluffer would do so; and the probability that a bluffer will fight must be between zero and one.

But the defender, whether bluffing or not, will decide whether to fight based on his expectation of the challenger's response. Let $(1 - n)$ be the probability with which, in equilibrium, the challenger is expected to withdraw from the war at its last move. Then if there is some probability that both a bluffer and a nonbluffer might fight a conventional war two conditions must hold (*nb*, nonbluffer; *b*, bluffer):

$$F_{nb} \leq (1 - n)W_{nb} + n\delta F_{nb} \tag{1}$$

$$L_b = (1 - n)W_b + n\delta L_b \tag{2}$$

Condition 2 is necessary because a bluffer that was not indifferent between fighting and not fighting would do one or the other with a probability of one. But we have seen that the probability with which a bluffer continues the war must be between zero and one. Note that one implication of condition 2 is that in equilibrium *n* must be between zero and one.

Conditions 1 and 2 imply in turn that

$$n = \frac{W_b - L_b}{W_b - \delta L_b} \leq \frac{W_{nb} - F_{nb}}{W_{nb} - \delta F_{nb}} . \tag{3}$$

Since $W = 1$ for both types of player,

condition 3 can be satisfied only if $L_b \geq F_{nb}$.

It might seem intuitively obvious that by resisting the challenger's attack, the defender has done something "tough" and thereby demonstrated something about his willingness to pay high costs in order to avoid defeat. But by choosing conventional resistance over a nuclear attack, the defender could also be said to have done something "soft," which might confirm the challenger's initial skepticism about his willingness to resort to a nuclear attack. Only if condition 3 holds is the first inference warranted and not the second. Let us see under what circumstances condition 3 might be expected to be true.

The difference between a bluffer and a nonbluffer is simply that for a nonbluffer the expected value of a nuclear counterforce attack is greater than the value placed on a Soviet victory, while for a bluffer the reverse is true. Suppose, then, that a nonbluffer has this preference because of being certain that the Soviets will not respond to a counterforce attack. If so, then with our assumptions condition 1 could never hold, since an immediate counterforce attack would be certain to preserve the independence of Europe, while fighting a conventional war would do so only if the Soviets eventually withdrew. But Soviet withdrawal could not happen with certainty (since in equilibrium n must be greater than zero), and there would be significant costs associated with the conventional military conflict that would occur if they did not. Of course, if U.S. leaders preferred to preserve Europe's independence without damaging the Soviet Union, then for some n, condition 1 might hold; but there would still be no reason why condition 3 would also have to be true.

Suppose, on the other hand, that the expected consequences of all alternatives are the same for both bluffing and nonbluffing defenders but that bluffers are simply more risk-averse than nonbluffers, so that the utilities they assign to these consequences are different. In that case, if the utilities of both types of defender are measured on the unit interval, then for every outcome between the most and least preferred, the utility values of bluffers will be higher than the utility values of nonbluffers.[7] Then $L_b > F_b > F_{nb} > L_{nb}$; therefore, condition 3 will be satisfied.

Thus, if fighting a conventional war in Europe is to convey information about the willingness of the United States to resort to a nuclear counterforce attack against the Soviet Union, it must be common knowledge that U.S. leaders are uncertain of the consequences of such an attack. Otherwise, if a Soviet conventional attack on Europe is not met immediately by a nuclear counterforce attack by the United States, Soviet leaders will, with our assumptions, *reduce* the probability they assign to the possibility that the United States would resort to such an attack.

Note, as well, that it must also be common knowledge that a defender who was bluffing would have no incentive to fight unless fighting would increase the credibility of its threat to resort to a nuclear attack. Thus, increasing the probability that the West might win a conventional war in Europe diminishes the significance of conventional resistance as a signal of the willingness of the United States to use nuclear weapons.

With these assumptions, it would be a straightforward task to specify a sequential equilibrium for this game, which would obviously require both a bluffing defender and the challenger to adopt mixed behavioral strategies. Our discussion of the difference between bluffers and nonbluffers, however, makes it easy to state a more general set of assumptions that are much more plausible. We have seen that if continuation of the war is to convey information about the willingness of the defender to resort to a nuclear counterforce attack, then the difference

Table 2. Sequential Equilibrium with Nuclear Superiority

Strategies	Beliefs
C_1: quit if $L_C > e(C_1)$, otherwise conventional attack	$G_D(x)$ for $x = e(D_1)$
D_1: nonbluffers continue, bluffers quit unless $L_D \leq e(D_1)$	$n = G_C(b^*)/G_C(x)$ for $x = e(C_1)$
C_2: quit if $L_C > b^*$, otherwise conventional attack	$b^* = 1 - [G_D(L_{crit})/G_D(x)]$ for $x = e(D_1)$
D_2: bluffers quit, nonbluffers counterforce attack	—

between bluffing and nonbluffing defenders must be the result of their attitudes toward risk. Instead of assuming that it is common knowledge that a bluffer is characterized by one particular set of utilities and a nonbluffer by another, therefore, we can more plausibly assume that the defender might be characterized by any of a continuum of possible utility functions, arrayed in order of increasing risk aversion.

Then, given p, there is some critical value of L (L_{crit}) such that any defender for whom L is greater than this critical value will prefer L to F and any defender for whom L is less than this value will prefer F to L. If we assume that the challenger's uncertainty about which utility function might characterize the defender can be represented by a continuous distribution function, G_D, then b (the probability that the defender is bluffing) is simply $1 - G_D(L_{crit})$, that is, the probability that $L_D > L_{crit}$. Similarly, let us assume that the defender's uncertainty about the challenger's utilities can be represented by the distribution function G_C.

In equilibrium, choosing conventional war has a well-defined expected value. Let $e(C_1)$ denote the expected value of conventional war at the challenger's first choice node—and similarly for the other two nodes at which conventional war might be chosen. Note that if the challenger chooses to continue the conventional war at its second choice node it will receive its most preferred outcome with a probability of b^* (its updated probability that the defender is bluffing) and its least preferred outcome with a probability of $(1 - b^*)$. Thus, $e(C_2) = b^*$.

It is still true that in equilibrium, continuation of the war cannot leave the challenger certain that the defender is not bluffing. Thus, there must be at least one possible bluffer for whom $L_b \leq (1 - n)W_b + n\delta L_b$. But since $\min(L_b) > \max(L_{nb})$, if any bluffer does not prefer quitting to continuing the war, then all possible nonbluffers must prefer continuing the war to an immediate counterforce attack. Thus, in equilibrium, the probability that a nonbluffer will continue the war must be one. Therefore, the only sequential equilibrium in this game consists of the behavioral strategies presented in Table 2, along with the associated beliefs.

Note that at each node, each player simply acts so as to maximize his expected utility, given his beliefs and that the beliefs represent the appropriate conditional probabilities, given the behavior prescribed by the equilibrium. At the final choice node, the defender will quit if bluffing and launch a nuclear counterforce attack if not. At the preceding move, the challenger will use the defender's previous behavior to update the probability that the defender is bluffing and use that updated probability to calculate the expected utility of continuing the war rather than quitting. Thus, the challenger's belief at C_2 is represented by the probability that $L_D > L_{crit}$, given that $L_D < e(D_1)$. Similarly, at his first move the defender will use the challenger's decision to attack to update the estimate of the challenger's degree of risk aversion and thus the probability that the challenger will quit rather than continue the war; using this probability, the defender then calculates the expected utility of conventional resistance. And the challenger, in deciding whether to attack, uses the distribution function over the defender's utilities to calculate the expected utility of attacking.

Since the point of nuclear counterforce strategies is to influence the challenger's initial move, let us look at it more closely. If both b and the challenger's utilities are common knowledge, then it cannot be an equilibrium for the challenger to attack at its first move unless $L_C < b$, since otherwise it will be common knowledge that the challenger will quit at its second move; and therefore the defender will certainly resist. If there is uncertainty about the challenger's utilities, however, then it cannot be common knowledge at the outset that the challenger would quit at his second move. Thus, a challenger might attack even though already persuaded of the credibility of the defender's threat sufficiently to be deterred from continuing the war at his second move.

The defender's option of responding to an attack with conventional weapons therefore weakens the deterrent effect of his nuclear threat, in the sense that some possible adversaries would be deterred if the option did not exist but are not deterred if it does exist. The option of conventional resistance, however, provides the defender with a means of enhancing the credibility of its nuclear threat after an attack has occurred—for those adversaries for whom the nuclear threat alone would be insufficient to deter a conventional attack. Thus, this model illuminates the relation between the two components of the McNamara Strategy—the no-cities doctrine and the doctrine of flexible response—as well as the controversy that ensued about the effects of this strategy on NATO's ability to deter a Soviet conventional attack.

Nuclear Parity

The preceding discussion implies that a nuclear counterforce capability can have a deterrent effect even if it is common knowledge that a counterforce attack would be a very risky option to choose—as long as it is not common knowledge that the probability that one would resort to it is zero. This provides us with the basis for a definition of a minimally credible counterforce capability: a counterforce capability is minimally credible if it is not common knowledge that the probability that one would resort to it is zero. Thus, one definition of nuclear superiority in a situation of mutual assured destruction is that nuclear superiority consists of having a minimally credible counterforce capability when one's adversary does not.

By this definition, nuclear parity is a situation in which both sides have minimally credible nuclear counterforce capabilities. And it is when nuclear parity in this sense exists that the problem of first-strike incentives arises. To see what difference this makes, let us now assume that $p_C > 0$, so that the probability that the challenger prefers L to F is not one (as in the preceding discussion) but b_C ($0 < b_C < 1$). Note that this change is not the result of any alteration in the distribution of the challenger's possible utilities but of a change in the probability that the challenger might prevail as a result of a nuclear counterforce attack. Let us see what effect this has on the solution to the game.

As before, I assume that players who prefer a nuclear counterforce attack to losing are uniformly less risk-averse than players with the opposite preference; otherwise, it is not possible for continuation of the war to enhance the credibility of one's nuclear threat. This implies that at every choice node there are only two types of equilibrium behavior: either all bluffers quit (while nonbluffers may continue or preempt) or all nonbluffers continue (while bluffers may continue or quit).

The following proposition states another, less obvious restriction on the types of equilibrium behavior that can exist in this game:

PROPOSITION 1. *If, in equilibrium, some types of nonbluffing players are expected to choose a nuclear counterforce attack at the second or third choice nodes in this game, then all types of nonbluffing players must be expected to do so.*

Proof. Suppose, to the contrary, that there is, in equilibrium, a b_j^* such that at either of these choice nodes belonging to player *i*, there is some probability that a nonbluffing *i* might choose conventional war and some probability that a nonbluffing *i* would choose a counterforce attack (and thus no probability that a bluffing *i* would choose conventional war). This could only be true if at the preceding move there was some positive probability that a bluffing player *j* would choose conventional war (since, if not, the player at the choice node in question would be certain that the opponent was not bluffing, and therefore all nonbluffers would choose a counterforce attack). Thus, given player *i*'s expected behavior based on $b_j^* < b_j$, the probability that some *j* who was bluffing would choose conventional war must be just enough to justify player *i*'s expectation; and therefore, given any $b_j > b_j^*$, the probability that some *j* who was bluffing would choose conventional war must be too low to support b_j. But reducing such a b_j to b_j^* would reduce the probability that a bluffing player *j* would choose conventional war still further (because decreasing b_j would increase the likelihood of a counterforce attack by player *i*). Thus, if b_j is not an equilibrium expectation, then b_j^* cannot be an equilibrium expectation either; and equilibrium requires that all nonbluffing *i*'s choose a nuclear counterforce attack.

Proposition 1 implies that there are only two types of equilibrium behavior at the second and third choice nodes: (1) all bluffers quit and all nonbluffers launch a counterforce attack or (2) all nonbluffers choose conventional war and bluffers either quit or choose conventional war. Examples of sequential equilibria involving each of these types of behavior are presented in Table 3.

If b_D is less than F_C for even the least-risk-averse-possible challenger, a challenger would never choose conventional war at its initial choice node but would instead choose either not to challenge or to launch a nuclear counterforce attack. Given the prevailing skepticism about the credibility of counterforce strategies, this seems implausible; therefore the possibility of a conventional war cannot be ruled out. Thus, only examples of sequential equilibria when b_i is above this level for both players are presented in Table 3. One can easily verify that given the stated conditions and the beliefs at each choice node, each player is maximizing his expected utility and the beliefs at each choice node represent the appropriate conditional probabilities, given the prescribed behavioral strategies.

Note that in the first type of equilibrium, no bluffing challenger is expected to initiate a conventional war; thus, if a conventional war occurs, the defender will be certain the challenger is not bluffing and will therefore either not resist or launch a nuclear counterforce attack.[8] We have seen that when only the defender had a minimally credible counterforce capability, the only sequential equilibrium was a pooling equilibrium, that is, an equilibrium in which the challenger is still uncertain as to the true preferences of the defender. Somewhat surprisingly, one of the effects of parity in counterforce capabilities is to create the possibility of a separating equilibrium, that is, an equilibrium in which the true preferences of one of the players are fully revealed.

The reasoning supporting proposition 1 implies that there is no exact counterpart to this pooling equilibrium for the defender. However, it is possible that resistance by the defender will dissuade all challengers from continuing the war even if $b_D{}^*$ is greater than zero, as the second type of equilibrium illustrates. The third type of equilibrium illustrates the possibility that even challengers for whom the defender's nuclear threat is initially very credible may nonetheless initiate a conventional war while planning to quit if the defender resists. Finally in the fourth type of equilibrium, a counterforce attack

Table 3. Examples of Sequential Equilibria with Nuclear Parity

	Strategies	Beliefs
	1. $b_D < L_{\text{crit}_C}$	
C_1:	bluffers quit, nonbluffers conventional attack if $e(C_1) = b_D \geq F_C$	b_D
D_1:	bluffers quit, nonbluffers counterforce attack	$b_C{}^* = 0$
C_2:	continue conventional war if $b^* \geq F_C$, otherwise counterforce attack	$b_D{}^* \leq b_D$
D_2:	bluffers quit, nonbluffers counterforce attack	—
	2. $b_C > L_{\text{crit}_D}$	
C_1:	nonbluffers conventional attack, bluffers quit unless $L_C \leq e(C_1) = b_D + (1 - b_D)\delta L_C$	b_D
D_1:	nonbluffers conventional resistance, bluffers quit unless $L_D \leq e(D_1) = b_C{}^*$	$b_C{}^* = 1 - [G_C(L_{\text{crit}})/G_C(x)]$ for $x = e(C_1)$
C_2:	bluffers quit, nonbluffers counterforce attack	$b_D{}^* < F_C$ for all C
D_2:	bluffers quit, nonbluffers counterforce attack	—
	3. $b_D < L_{\text{crit}_C}$ and $b_C > L_{\text{crit}_D}$	
C_1:	nonbluffers conventional attack, bluffers quit unless $L_C \leq e(C_1)$	b_D
D_1:	nonbluffers conventional resistance, bluffers quit unless $L_D \leq e(D_1) = b_C{}^*$	$b_C{}^* = 1 - [G_C(L_{\text{crit}})/G_C(x)]$ for $x = e(C_1)$
C_2:	bluffers quit, nonbluffers counterforce attack	$b_D{}^* \leq b_D$
D_2:	bluffers quit, nonbluffers counterforce attack	—
	4. $b_D > L_{\text{crit}_C}$	
C_1:	nonbluffers conventional attack, bluffers quit unless $L_C \leq \min e(C_1)$	b_D
D_1:	nonbluffers conventional resistance, bluffers quit unless $L_D \leq e(D_1)$	$b_C{}^* = 1 - [G_C(L_{\text{crit}})/G_C(x)]$ for $x = \min e(C_1)$
C_2:	nonbluffers continue, bluffers quit unless $L_C \leq e(C_2) = b_D{}^*$	$b_D{}^* = 1 - [G_D(L_{\text{crit}})/G_D(x)]$ for $x = e(D_1)$
D_2:	bluffers quit, nonbluffers counterforce attack	—

Note: Spanner heads represent necessary conditions for existence.

occurs only if the defender has to make a final choice and is not bluffing. This type of equilibrium is similar to the sequential equilibrium identified in the situation in which only the defender had a nuclear counterforce capability.

Note that both the first and the last types of equilibrium might exist in the same game. If the defender was expected to interpret any conventional attack as conclusive evidence that the challenger was not bluffing, the defender would have to quit or launch a nuclear counterforce attack at its first move; and if this reaction by the defender is expected, this interpretation of the challenger's behavior would be correct. On the other hand, if the defender was expected to interpret a conventional attack as compatible with the incentives of a bluffing challenger, this expectation would also be self-confirming.

This paradoxical result can be avoided if one assumes that it is empirically implausible that the probability that the defender is bluffing could ever be low enough that it was common knowledge that all bluffing challengers would prefer not to attack (and similarly for the probability that the challenger is not bluffing). If so, then it also seems reasonable to assume that it could not be common knowledge that all bluffing challengers would choose to quit at the challenger's second move. Then only the fourth type of equilibrium can exist. Therefore, the solution to the game is identical to the solution when only the defender had a nuclear counterforce capability. But this implies that the challenger's counterforce capability has no effect.

This counterintuitive conclusion is the result of two features of the situation that tend to be ignored in debates about the relation between counterforce capabilities and the incentive to preempt. First, there is an asymmetry between the defender and the challenger, in that the challenger expects to win a conventional war, whereas a nuclear counterforce attack is the only way the defender can hope to avoid a loss. For the challenger, therefore, both a preemptive counterforce attack and continuation of the war are gambles in which there is some probability of winning and some probability of instead being the victim of a nuclear attack. Second, the challenger who really prefers a nuclear counterforce attack to losing must not be very averse to risk. Thus, the challenger who is willing to accept the risk associated with a preemptive nuclear counterforce attack may for the same reason choose instead to accept the risk associated with continuing the war. This is why the defender's success in inducing the challenger to take the threat of a nuclear attack more seriously can induce risk-averse (and therefore bluffing) challengers to quit and not induce nonbluffing (because risk-acceptant) challengers to preempt.

Even so, the conclusion that there is no danger that the challenger will preempt rests on the assumption that it is common knowledge exactly what value of b_D leaves the least risk-averse bluffing challenger just indifferent between continuing the war and preempting. This assumption also seems empirically implausible. If we assume, instead, some uncertainty concerning exactly what this critical value of b_D is, then downward revision of b_D will lead to an increase in the probability that bluffing challengers will quit but may also imply some probability that nonbluffing challengers will decide to preempt.[9]

Note the similarity between this result and so-called brinkmanship models of conflict. Brinkmanship models are based on Thomas Schelling's notion of threats-that-leave-something-to-chance and assume that while no decision maker would deliberately choose to initiate a nuclear attack, there is nonetheless some probability that any confrontation between nuclear powers will result in an accidental nuclear war. The source of this autonomous probability of war is usually not specified, however; thus, brinkmanship models leave unclear how a war can occur without anyone's choosing to engage in it (Powell 1987, 1988; Schelling 1966, 92–125).

In the model developed here, however, conventional war leads to an increase in the probability of nuclear war not because it increases the chance of some unspecified accident but because waging a conventional war can signal one's willingness to resort to nuclear weapons, if necessary, to avoid defeat. And counterforce capabilities perform the function of making the threat to do that minimally credible.

The Incentive To Strike First

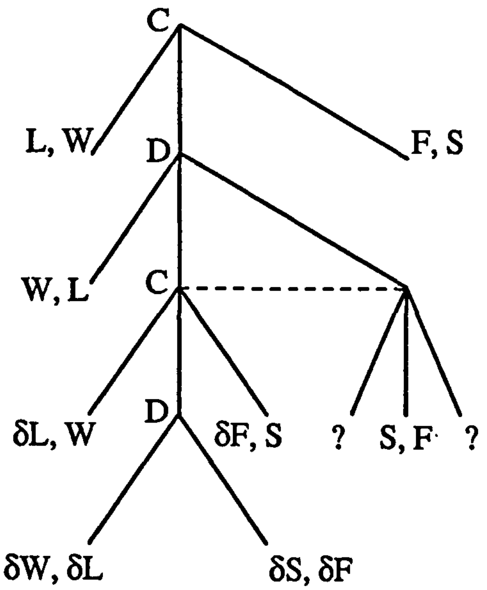
Discussions of the relation between counterforce strategies and crisis stability often do not distinguish between two types of incentive to strike first. One type is examined by Powell (1989a): because of the damage-limiting effects of a first strike, each state prefers a nuclear war that it initiates to suffering the enemy's first strike; but neither state would ever choose a nuclear attack if it were certain that the other would not attack. In such situations, nuclear war, if it occurs, could only be the result of a tragic mistake; thus, both sides have a common interest in avoiding situations in which such a mistake might occur.

The other type of first-strike incentive is represented in the model already discussed: because of the effects of counterforce attacks on the victim's incentive to retaliate, even if a state is certain that the other would not attack, no one can be sure that it would not choose a nuclear attack nonetheless, if that were the only alternative to defeat in a major conventional war. But in order for such a desperate move to have any chance of success, a state that resorts to it must attack first. And therefore a state that is willing to use nuclear weapons has an incentive to attack before the other has an opportunity to do so. Thus, a bluffing defender's conventional resistance might provoke a preemptive attack by the challenger if it convinced the challenger that the defender was not bluffing.

In this model, however, neither state would launch a preemptive nuclear attack if it was bluffing. Thus, the danger of preemptive attack by the challenger is really identical to the danger that the challenger faces in launching a conventional attack when only the defender has a nuclear counterforce capability: just as the challenger's conventional attack may provoke a nuclear response from the defender if the defender is not bluffing, so the defender's conventional resistance may provoke a nuclear response from the challenger if the challenger is not bluffing.

It is important to note that this conclusion takes into account the possibility that conventional resistance by the defender may itself entail some degradation of the challenger's nuclear counterforce capabilities and thus incorporates at least one possible interpretation of Posen's discussion of "inadvertent nuclear war" (Posen 1982). The model assumes, however, that each state always makes its decisions with full knowledge of what the other state did at its preceding move. One effect of this assumption is that whenever a state has the option of preempting, it also has the option of quitting the conflict, which implies that bluffers will never launch a nuclear attack. But (as noted earlier) another interpretation of the scenarios leading to inadvertent escalation discussed by Posen and others emphasizes that the challenger may not be able to distinguish between conventional resistance and the beginnings of a nuclear counterforce attack. And if the antagonists are unable to tell whether the other has already attacked, then the first type of first-strike incentives may become significant. Thus, it is important that we investigate what effect this sort of limitation will

Figure 4. Retaliation with Independent Moves



have on the solution to the game just analyzed.

It seems reasonable to assume that the Soviet Union could attack Western Europe in such a way that U.S. leaders would be confident the Soviets had not simultaneously launched a nuclear attack against U.S. territory. If so, it also seems reasonable to assume that if the United States decided to concede defeat rather than resist, Soviet leaders would have no doubt that that was what the United States had done. Thus, the most plausible modification of the game discussed above is to assume that if the United States rejects nonresistance at its first move, the Soviets will be unable to tell whether the United States has chosen to launch a nuclear attack immediately or to offer conventional resistance in the hope that the threat of its attack will deter the Soviet Union from continuing the war.

Such a modification in the structure of the game is depicted in Figure 4. The line drawn between the two choice nodes at the challenger's second move indicates that the challenger cannot distinguish between them and thus does not know whether the defender has chosen to attack or to offer only conventional resistance. I assume, however, that if the United States has chosen conventional resistance, Soviet leaders can continue the war in Europe in such a way that U.S. leaders will be confident that the Soviets have not launched a nuclear attack on the United States.

In order to complete the specification of this modified game, we must state the effect of our new assumptions on the consequences of the challenger's alternatives at his second information set. It will be convenient to consider this question by looking at the normal form of the subgame that begins with the defender's first information set, which is depicted in Figure 5. (In Figure 5, strategy "Q" stands for "Quit," "C" for "Continue," and "A" for "Attack.")

If the defender has chosen conventional resistance, then the challenger's alternatives will clearly have the same effects as before. Moreover, if the challenger chooses to continue the conventional war at his second information set, then the effects of the defender's choices will also be the same as before. To understand the implications of our new assumptions, therefore, we must specify their effect on the challenger's decision to quit or to launch a nuclear counterforce attack at his second information set, if the defender has already chosen a nuclear counterforce attack (the payoffs for these outcomes are designated by question marks in Figures 4 and 5). There is a wide range of possibilities, and the literature on this subject offers little guidance as to which is the most appropriate.

As noted earlier, one effect of these new information conditions may be to make the challenger pessimistic about the possibility of communicating to the defender

Figure 5. Independent Moves in Normal Form

		Defender			
		Q	A	CQ	CA
Challenger	Q	W, L	?	δL, W	δL, W
	C	W, L	S, F	δW, δL	δS, δF
	A	W, L	?	δF, S	δF, S

his willingness to withdraw from the war in time to induce the defender to halt his attack. This implies that the challenger cannot be sure of getting L_C by quitting. But the fact that the defender has already begun his nuclear counterforce attack must also imply some degradation in the value of F_C if the challenger decides instead to resort to a nuclear counterforce attack of his own.

Note that if quitting is not to dominate a nuclear counterforce attack for a bluffing challenger even with the new information conditions, then if the defender is assumed to have already chosen a nuclear attack, a bluffing challenger must strictly prefer the consequences of a nuclear counterforce attack to the consequences of quitting. Since a bluffing challenger prefers L_C to F_C, however, if it is to prefer a nuclear attack to quitting, then the probability of getting L_C rather than S_C by quitting must be less than the probability of getting F_C rather than S_C by launching a nuclear counterforce attack. More specifically, since S_C can be assigned the value of zero, if the challenger is to prefer a nuclear attack to quitting (given that the defender has already chosen a nuclear attack), then the ratio of the factor by which L_C is discounted to the factor by which F_C is discounted must be greater than the ratio of L_C to F_C.[10] If this is not true, then even with our new information conditions, no one who is bluffing will launch a preemptive nuclear counterforce attack.

There may be circumstances in which this condition holds. It is not implied by the existence of counterforce capabilities alone, however. And it seems possible to avoid creating circumstances in which it might be important, for example, by avoiding the sorts of ambiguous attacks discussed by Posen. (We have already seen that such attacks are not necessary for conventional resistance to be used to enhance the credibility of the defender's nuclear threat, contrary to Posen's assumption.)

Powell (1988a) has shown that if, instead of preempting, states have the option of trying to quit the conflict and if it is common knowledge that only first-strike incentives of the first type exist, no state will preempt. The analysis offered here implies that even if it is not common knowledge that only first-strike incentives of the first type exist, under similar conditions no state will preempt unless it was also prepared to launch a nuclear attack in order to avoid defeat in a conventional war.

In any model of first-strike incentives of the first type, crisis stability is a criterion for evaluating policy that is exogenous to the game: if the players in the game can be shown to have an incentive to preempt, then that is a reason for changing the rules of the game, not for changing one's behavior in the game. In the games analyzed here, however, the policy implications of the danger of preemption are modeled in the game itself: if the probability that the defender's conventional resistance will provoke a preemptive attack by the challenger is too high, then a bluffing defender whose aversion to risk is "too great" should not resist (as long as conventional resistance has no chance of success); but others may choose to do so. Thus, as long as states do not adopt strategies that make their adversaries pessimistic about their ability to signal their willingness to back down, an argument against counterforce strategies based on the enemy's incentive to strike first must assume that the challenger's threat to initiate a nuclear war rather than concede defeat is more credible than most criticisms of counterforce strategies themselves seem to imply. And whether one accepts the argument or not depends upon one's attitude toward risk.

Conclusion

My purpose here has not been to justify the use of nuclear counterforce strategies but to show that they are not necessarily inconsistent with rational behavior, despite the assertions of several writers (e.g., Jervis 1984; Kull 1988). One reason for doubting the rationality of counterforce strategies is the fact that no feasible counterforce attack can disarm the enemy; thus, the victim of such an attack must still have the capability of doing unacceptable damage to its initiator. In the models I have presented, however, while counterforce attacks cannot deprive the enemy of the capability of retaliating, they can provide him with a powerful incentive not to do so.

Critics of this sort of justification of counterforce strategies have argued that decision makers could not have enough confidence in this effect to warrant launching such an attack. In my models, however, the function of counterforce capabilities is merely to lead the adversary to believe that the probability that one might resort to such an attack as a means of avoiding a major defeat is greater than zero.

Critics of such "perceptual" justifications of counterforce strategies have claimed that they assume nonrational behavior and are therefore entirely arbitrary. In my models, however, the perceptual effects of counterforce strategies are justified by the concept of a sequential equilibrium, a well-defined definition of rational behavior in games with incomplete information.[11]

Another criticism of such "perceptual" justifications is that because of the first-strike incentives created by counterforce strategies, even if they are effective, they may lead to inadvertent nuclear war rather than deterrence. Most discussions of first-strike incentives are flawed in two respects, however: they ignore the possibility that states may elect to withdraw from a conflict rather than launch a nuclear attack, and they confuse two different kinds of first-strike incentives. Powell (1989a) showed that if the only motive to strike first is to limit the damage from another's attack and if states also have the option of trying to quit a conflict, then (if other plausible conditions are also met) the mere existence of first-strike incentives of this type will not lead to nuclear war. In my models (in which states willing to use counterforce strategies have an additional incentive to attack first), under similar conditions no state will launch a nuclear attack unless it prefers the risks associated with such an attack to accepting defeat. Thus, anyone who is skeptical about the willingness of states to resort to nuclear war rather than accept defeat must also be skeptical about the possibility that a nuclear war will occur inadvertently.

Since the point of counterforce strategies is to give oneself an incentive to resort to nuclear war, in my models the probability that a nuclear attack will occur is not zero. The implications of this fact for policy, however, depend on one's estimate of the probability that one's adversary would actually be willing to initiate a nuclear attack, as well as one's own attitude toward risk. Moreover, willingness to subject oneself to such risks can enhance the credibility of one's own threats, and thus increase one's ability to deter an opponent if he is bluffing about his willingness to launch a nuclear counterforce attack.

Another criticism of counterforce strategies is that even without them, there is sufficient likelihood that conventional war would escalate to a nuclear conflict for nuclear weapons to have an extended deterrent effect. If neither side uses counterforce strategies, however, it is not clear what the basis for this likelihood is.

It is sometimes suggested that nuclear war might occur accidentally. But since a war requires the participation of at least two states, it is not obvious how even a nuclear war could be fought accidentally. An accidental all-out first strike by one side against the other seems impossible. Thus, the most plausible scenario by which an accidental nuclear war might occur is that an unintended incursion or unauthorized limited attack by one side is interpreted by the other as part of a first strike and thus leads to a retaliatory attack by the victim; this retaliatory attack is then interpreted in turn by its victim as a first strike, and therefore leads to a retaliatory attack by the first state. A necessary link in this chain of events is the initial mistaken retaliation by the target of the accidental incursion.

The only difference between this scenario and the two types of preemptive attacks just discussed, however, is that this mistaken retaliatory attack is preceded by some unauthorized action by the enemy (or the mistaken belief that such an action has been taken, based perhaps on some computer malfunction). This scenario thus tacitly assumes that the state considering whether to retaliate after the initial accidental or unauthorized event is far more optimistic about the consequences of a nuclear attack than about the consequences of trying to quit the conflict. But if this state does not have counterforce capabilities, it is not clear why this should be true.

Thus, if states do not use counterforce strategies, it is not clear what basis there is for assuming that conventional war carries with it some autonomous risk of accidental nuclear war. On the other hand, we have seen that even when there is no risk of accidental nuclear war (in the sense just discussed), conventional war can imply an increasing probability of nuclear war that is the result of each state's updating some nonzero probability that the other state might be willing to launch a counterforce attack rather than accept defeat. When the probability of nuclear war is explicitly derived rather than merely assumed, therefore, the difference between so-called brinkmanship models of extended deterrence and deterrence strategies that rely on counterforce capabilities may not be as great as is commonly believed.[12]

Moreover, if one believes that nuclear threats, to be credible, must entail the possibility of accidents or inadvertent escalation, then the price of making conventional war safer is that extended nuclear deterrence will be weakened.[13] We have seen, however, that counterforce strategies need not rely on accident or inadvertence to make the threat of nuclear war credible.

Critics of counterforce strategies have sometimes argued that the threat of fighting a conventional war in Europe was alone enough to deter the Soviets, so that we need not have worried about the credibility of extended nuclear deterrence. Pessimism about NATO's ability to win a conventional war in Europe, however, could create doubts about the credibility of even conventional resistance to a Soviet attack. Thus, it is important to note that one effect of counterforce strategies in the models presented is that they provide a rational motive for waging a conventional war even when one expects to lose it.

Notes

I owe a great deal to a very interesting working paper by Michael McGinnis (1988), some of whose analytical framework I have borrowed but who takes a different position on some of the issues discussed here. I would also like to thank Robert Powell and James Fearon for commenting on earlier drafts. An earlier version of this paper was presented at the annual meeting of the American Political Science Association in San Francisco in September 1990. I thank Norman Frohlich for his comments as the panel discussant at that meeting.

1. Bear in mind that in the case of extended deterrence of conventional attacks, the defender, not the challenger, must be the first to launch a nuclear attack.

2. Most discussion of this issue has focused on the three-stage scenario represented in Figure 2, which assumes that the victim must reply to a counterforce attack with an all-out countervalue response. But a comparison of Figures 1 and 2 shows that all that is really necessary is that the state attacking first also has the option of attacking last. This could be because in any sequence of attacks it will be the first state with nothing left to lose (Powell 1989b) or because it will be the last state with something left to attack with. Counterforce attacks can thus have two purposes: they can alter the balance of forces in favor of the state attacking first, so that in any subsequent series of attacks the enemy's forces will be exhausted before the attacker's; and they can destroy the most accurate forces available to the enemy, so that it can only reply with highly destructive countervalue attacks, with the result that the attacker will be the first with nothing left to lose.

3. Of course, even if it did, one could argue that the enemy was already deterred without it, or that the increase in credibility was not worth the cost.

4. This is true even if one takes into account the option of launching under attack, which is not as decisive an answer to counterforce strategies as many critics assume. For a discussion of some of the complexities, see Steinbruner 1984.

5. The model developed is based directly on the model discussed in McGinnis 1988.

6. Since in this case there is uncertainty only about the defender's preferences, there is no need for subscripts to identify which player b refers to.

7. When utilities are measured on the unit interval, the utility assigned to an intermediate outcome is simply the probability of winning a lottery over the best and worst outcomes that leaves a person indifferent between that lottery and the certain prospect of the outcome whose utility is being measured. More risk-averse people would by definition require a greater probability of winning than less risk-averse people, if they are not to prefer the certain prospect.

8. In the first type of equilibrium, the game does not proceed past D_1. The concept of sequential rationality nonetheless requires that out-of-equilibrium strategies and beliefs be specified. Since C_2 is reached with a probability of zero, however, player 2's beliefs at that node are arbitrary.

9. Given the other assumptions we have made, this uncertainty could most plausibly be modeled as the result of uncertainty concerning the value of p_C.

10. Note that whether this is true or not is independent of the probability that the defender has in fact chosen a nuclear attack.

11. Thus, the analysis offered here provides an alternative to Kull's interpretation of the remarkably candid discussion by his respondents of the "perceptual" bases for U.S. declarative military strategy (Kull 1988). Kull claims (without much supporting argument) that such justifications of counterforce strategies assume nonrational behavior. See also the discussion in Sabin 1987.

12. Betts uses the metaphors of Russian roulette and chess to distinguish between these two approaches to extended deterrence (1987, 11–16).

13. Posen (1982) assumes such a dilemma. Nalebuff (1986) has used Posen's discussion of inadvertent escalation to justify a model of brinkmanship in which conventional war entails a continuously increasing autonomous risk of nuclear war. Like Posen, he assumes that the credibility of nuclear threats requires the possibility of accidental or inadvertent escalation and concludes that since

making conventional war safer will also make it more likely, the probability of nuclear war cannot be reduced by measures that would make such accidents less likely.

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EUROPEAN PARTY LOYALTIES REVISITED

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The likelihood that party loyalties reflect the characteristics of parties and political competition within specific political environments was generally ignored in critiques of party identification abroad. In this article, I identify substantial frequencies of stable partisan ties among British, Dutch, and German voters in the 1960s through the 1980s. Stable party loyalists in turn usually vote for their preferred party in subsequent elections. Long-term partisan ties are most common among supporters of traditional cleavage parties. These long-term party loyalties reflect the effects of both long-standing hostilities toward opposing parties and well-developed party principles. Since partisan responses are internally consistent and subsume ideational as well as affective components, these feelings resemble social psychology's affect-laden schemata.

Slightly more than a decade ago European scholars severely criticized the utility of the American concept of party identification for understanding European voting. This critical literature largely ignored the possibility that a European version of party loyalties might exist reflecting the influence of political environments that were different from the example of the United States. The intuitively appealing possibility that distinctive political contexts produce distinctive types of partisanship was not normally considered.

The research I report here examines European psychological partisanship using recent survey data from Britain, Germany, and the Netherlands. In each country, I discover that many persons are stable partisans. Moreover, very few of these stable partisans defect from their party ties in later elections. My analysis demonstrates that this high degree of conformity occurs because many voters' long-term party ties are buttressed by stable views toward long-term party principles and/or stable party images; many voters' loyalties are further fortified by negative views of opposing parties. This constrained structure of responses to parties makes European partisanship resemble an affect-laden schema.

Reflecting the effects of political context, partisan schemata and stable voting tendencies are best developed among followers of old cleavage parties. Supporters of noncleavage parties tend to be less stable partisans and lack fully developed partisan attachments. Older, cleavage-based styles of interpartisan hostility and ideological competition are thus echoed in today's voters' psychological partisanship even though the direct effects of traditional social cleavages have themselves declined. By reestablishing the importance of long-term party loyalties, and describing their properties and relationships to other partisanship elements, a view of electoral psychology suitable to European experience is proposed.

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The Setting: The Perceived Disutility of Party Identification amid Perceptions of Change

Research has generally found U.S.-style party identification wanting in a European setting. Greater tandem movement between elections in party identification and the vote relative to the U.S. case (Borre and Katz 1973; Butler and Stokes 1969; Kaase 1970; LeDuc 1981, Thomassen 1976) resulted in doubts about party identification's utility as both a long-term attitude and a motivation independent from the voting act. A very strong correlation between party identification and voting choices in cross-sectional analyses led to additional questions about the concept's utility, as well as doubts about its measurement (Budge, Crewe, and Farlie 1976). The failure of party identification "theory" to account for lowered turnouts, increased swings in the vote, and greater support for minor parties in Britain (Crewe 1976) was a third element of its weakness, according to its critics.

Only a handful of scholars (Converse and Pierce 1986; Heath and McDonald 1988; LeDuc 1981; Norpoth 1978) have continued to treat party identification as a major concept in electoral behavior analysis in the European setting, although a number of scholars have used it in studies of partisan change (e.g., Crewe 1985; Haerpfer 1985; Klingemann 1985; van der Eijk and Niemoller 1985). An additional few persons (Barnes 1990; Cain and Ferejohn 1981; Richardson 1989) have explored the idea that specific party or party system characteristics contribute to distinctive types of partisanship. Cain and Ferejohn showed that British party loyalties looked more like the U.S. model when Liberal supporters, whose partisanship was weak and variable, were removed from analysis. Samuel Barnes and I have looked at the effects of specific party characteristics, such as size and age, on types of partisanship. I continue this theme here.

Preoccupation with change and electoral volatility has certainly been a second factor leading recent European voting research to be concerned with long-term ties like party identification less than in the past. Most people agree that European voting entered an era of increased volatility in the 1970s and thereafter (Borre 1980; Crewe and Denver 1985; Dalton, Flanagan, and Beck 1984; Maguire 1983; Pedersen 1983). Dramatic social change since World War II also generated new political preferences or encouraged volatility by removing the traditional social anchors of the vote. The emphasis on change and volatility was, however, only partly justified. Extreme examples of interelection fluidity (e.g., Denmark in 1971–73 or Britain in 1974) or major realignments (e.g., France in and after the mid-1950s) could be seen, usually as a response to changes in the parties themselves. In reality, aggregate party voting support remained relatively stable in recent decades in many countries even though there were also some episodes of increased volatility (Pedersen 1983). Interelection stability has consistently outweighed cross-election volatility at a ratio of four or five to one in most countries at most times. Partisan inertia has been as important an aspect of recent European electoral life as have change and fluidity.[1] These strong continuities in electoral patterns serve as a background to our search for stable individual level components of the vote.

Conceptual Perspective

Characteristics of parties and party systems influence the nature and shape of mass partisanship. American style party identification was the reaction by individual voters to bipartisan competition in a political setting where the main parties were both very old. In such an environment party loyalties to the two major parties could be stable over long periods of time and reflect inherited attitudes based on several generations of family partisan experience. At the same time, ideological conflict in the United States was fragmented and asymmetrical with party boundaries and of varying intensity depending on the time. Short-term voting deviations from long-term party ties in response to isolated high-profile issues and attractive presidential candidates (reflecting another systemic property) might occur as a result. Seeing issue opinions as an independent attitudinal response to discrete election campaign policies (as is done in the Michigan approach) thus fits the less tightly constrained U.S. political setting, with all of its ideological complexities, individualistic styles of leadership, and shifts in high profile issues over time. In contrast, European electoral politics has often been dominated by more coherent and symmetrical partisan and ideological cleavages. Tighter fit between party loyalties and elements of political belief could be expected in such an environment, as well as strong out-party hostility among supporters of opposing cleavage parties. Since sociopolitical cleavage-based ideological conflict and styles of competition were the most prominent traditional characteristic of these systems, it is likely that their polarizing influence will be seen in political attitudes today, even where the direct effects of social divides have themselves declined and issue content has in part changed. Finally, more than one type of partisanship could also occur reflecting different parties' varying relationships to cleavage systems or other specific party characteristics.

Thus, like all political attitudes, partisanship is a response to a particular set of political cues and alternatives (Crewe 1976; Goot 1972). Where systemic alternatives reflect traditionally deep societal divides, mutually opposing value systems and ideologies, and relatedly durable and consistent positiontaking by parties, the cognitive, affective, and evaluative portions of individuals' partisanship should be well developed and internally consistent. Moreover, high levels of interparty affective hostility between supporters of traditional cleavage parties should be found in systems where cleavage parties still maintain postures of strong mutual antagonism. Even where issue climates have changed to favor new, non-traditional-cleavage-linked political issues, the oppositional style and clarity of differentiation of traditional cleavage politics should ensure that partisan attitudes are more integrated than might be anticipated in the traditionally less clearly structured U.S. political environment.

The constrained partisan attitudes and strong out-party hostilities induced by cleavage-style electoral competition should, in my view, resemble the schemata of social cognition research.[2] This concept of party loyalties as a constrained cluster of interrelated bits of information, beliefs, and affective orientations stored in individual memory under a broad categorical label and retrieved in response to relevant stimuli (Fiske 1982) is further supported by several strands of mass political behavior research.

Recent research in both the United States and Europe has set the stage for seeing partisanship as an integrated schema consisting of both affective and cognitive components. Research on reciprocal causation (Jackson 1975; Markus and Converse 1979; Page and Jones 1979) has implied greater mutual interdependence between affective partisanship and other more purely cognitive responses than was foreseen by the pilot political psychology of the early 1960s.[3] Furthermore, Fiorina's (1981) widely valued concept of retrospective partisanship is itself also mainly a cognitive construct. In addition, recent British scholarship's view that many people subscribe to long-term party principles (Heath, Jowell, and Curtice 1985; Rose and McAllister 1986) implies a partisanship that incorporates substantial cognitive elements.[4]

In addition, since many scholars have felt that deep cleavages in European politics have made ideology important at both the elite and mass level (Almond 1956; Campbell and Valen 1961; Converse and Dupeux 1962), the internal knowledge structures called for by the schema approach are probably especially well developed for many European partisans.[5] Much the same structuring effects may also be found for party images, since images are responses to ideological symbols and conflict, as well as reactions to other aspects of parties.[6]

Recent European social and political change also has some important implications that qualify traditional views of partisanship in ways congenial to my approach. As is well known, traditional social cleavages are diminishing in importance in many places (e.g., Lipset 1981; Miller and Stouthard 1975; Montero 1986). This decline in the importance of environments, which can be assumed to have been relevant for early socialization into partisanship, is itself reflected in the remarkably low level of parental partisan influences reported in many countries, as well as in their decline over time in some places (see Appendix; also Rose and McAllister 1986). These tendencies make the traditional view that an inherited partisanship is a prior affective tie that influences later cognitive responses to issues and other political objects less credible. Partisanship today is much more likely to be derived from people's individual processing of political information than in the past. This being the case, mutually consistent affective loyalties, cognitive images, and long-term partisan value preferences are much more likely to be formed as part of a unified synchronic response to parties and other components of electoral politics than by a diachronic process in which affective loyalties precede and dominate all other elements of partisanship.

In my view, then, people learn about parties, develop images of them, and evaluate party policies using the same integrated mental framework. Because of these qualities, party loyalties comfortably fit the characteristics of an affect-laden schema (Fiske 1982; Fiske and Taylor 1984; Zajonc 1980). Stored in the structure are (1) positive affective feelings toward favored parties (termed *affective tags* by some researchers); (2) negative feelings toward nonfavored parties; (3) evaluative images favoring the supported party and not favoring other parties; and (4) political values and issue positions congenial to preference for a particular party.[7] Because of their structural interdependence, the components of partisanship's buttressing schema tend inherently to stability. Though partisanship may not be a lifelong commitment, considerable "intermediate" or long-term inertia in party loyalties is anticipated. For many persons, only major new events or especially salient issues can mount a discrediting challenge to these institutionalized ties.

I also assume that supporters of traditional cleavage parties might themselves have more stable opinions on long-standing divisive issues—and therefore better-developed partisan schemata—than supporters of parties where long-term principles were either less clearly established and/or less integrated into a common thread of political principles (Heath, Jowell, and Curtice 1985; Rose and McAllister 1986). Finally, many of our ideas about cleavage conflict and its effects on partisanship are probably most literally relevant for class conflict and its related ideological dimensions. Religious parties such as the Dutch Christian Democratic Appeal (CDA) or German Christian Democratic Union (CDU) may be somewhat less ideologically committed than traditional working class parties or both working and middle class parties in Britain's single-dimensional and largely secular party system, and these differences could affect the partisanship of followers of these parties. It is similarly possible that sizable differences in value commitments and imagery might be found among partisans in different countries, as well as among supporters of different parties. For example, sociopolitical conflict has been at times much more intense in postwar Britain than in Germany after the softening of the Social Democratic party's (SPD's) ideological stances in 1959, and the CDU's earlier embrace of a multireligious stance. These different experiences are relevant to the nature of partisanship in the different systems, as will be shown.

Method and Organization

The research I report here is based on secondary analysis of evidence from national election surveys from three countries—Great Britain, the Netherlands and the Federal Republic of Germany. All of the studies incorporated panel designs, which was essential for the purposes of my analysis. The most recently available national election studies from Britain (1983–87), Germany (1986–87), and the Netherlands (1981–82) contributed the base data sets.[8] Additional analyses, which employed data from studies conducted in the 1960s and 1970s, were used to supplement and validate the analysis of the base data findings. Neither the base surveys nor earlier election studies were equivalent in design or instrumentation across countries. Nor were studies in each country necessarily equivalent over time. Wherever possible, I have used variables and indexes based on equivalent measurements. But where measurement and indicator combinations vary significantly, this is indicated. (The Appendix summarizes these strategies.) Since the concern of this research is more with concepts, patterns, and relationships than with differences in levels or frequencies across time or countries, the equivalence problem is, hopefully, minimized.

The comparability of the three countries on a number of dimensions simplified the analysis in certain ways. Since all three countries had parliamentary forms of government, presidential system-parliamentary system differences were not a confounding element in the analysis (as they must have been in many comparisons of partisanship between the United States and European countries). Also, since each country had a clear multiparty system with roughly the same number of important parties, differences in party system complexity were not a visible problem. The fact that each party system contained two or three major parties representing traditional cleavages and one or two minor noncleavage parties actually permitted an unusual analysis of party type effects.[9]

Broad comparability of cases reduced some of the complexity inherent in comparative analysis and permitted attention to party type or party system effects that might otherwise have been obscured. There were also differences between the countries that were relevant for my analysis. Recent political experience was in no way identical across the three countries. New parties or electoral alliances had been formed in each country during or since the 1960s, and considerable volatility in electoral behavior had been observed reflecting these and other events. But this general pattern unfolded somewhat differently in the three countries. Traditionally opposing cleavage parties and ideologies continued to dominate electoral choices in Britain, although formation of a small Social Democratic party and a subsequent alliance between this party and a rejuvenated Liberal party presented a modified menu in 1980s elections there. In the Netherlands, the Democrats '66 movement had achieved "significant" minor party status by the 1980s, while the three traditional Christian cleavage parties united to form a common

Table 1. Stable Partisanship and Stable Partisan Voting

Stable Characteristic	Britain 1983–86	Netherlands 1981–82	Germany 1986–87
Party loyalties			
% stable T1–T2	72	52	52
T1–T2 correlation	.79	.71	.66
Stable vote			
% stable T1–T2	64	67	63
T1–T2 correlation	.73	.81	.63

Note: For data sources see Appendix. Both percentages and correlation coefficients are based on calculations that included all persons in the panel portion of the relevant samples. Percentages of stable partisans do not include stable nonpartisans, only persons who supported the same party at two points in time.

party—the CDA—in 1980 (following earlier erosion of support for the Catholic People's Party and the Christian Historical Union [CHU]). In Germany the Green party appeared as an election contender in 1980. Of the three countries Germany had the lowest degree of traditional interparty cleavage competition following formation of a nonsectarian Christian middle-class party, the CDU, in 1946 and moderation of the Socialist party's program after 1959. The greatest recent interelection volatility had been witnessed in Britain where net interparty shifts of 5%–6% were registered between 1979 and May 1983, and roughly 12% of the electorate shifted party preferences between 1970 and 1974. Fairly dramatic long-term shifts could also be seen in religious party support in the Netherlands during earlier periods.[10]

Electoral Attitude Stability

Stable Partisanship and Stable Voting

Table 1 contains evidence from the 1980s on the stability of party loyalties and voting in Britain, the Netherlands, and West Germany. Two simple observations emerge from the Table 1 findings. First, relatively comparable and reasonably high levels of partisanship or partisan voting stability can be seen in all three countries. Around two-thirds of the total population in each country voted stably and/or were stable psychological supporters of a political party.[11] Secondly, the tendency to stable partisanship is manifested in different ways in different countries. Explicit stable psychological partisanship occurred well below stable voting percentages in two of the country cases out of three. Only in Britain did psychological partisanship hold up as well over time as the vote. Before reaching closure, we must ask ourselves what drives the stable, habitual vote in the absence of explicitly revealed party ties. Nevertheless, habitual voting is evidence of a stable partisan commitment, even if I have not yet spelled out what form such a commitment actually takes.[12]

Stable Partisanship Versus Issue Opinions and Party Images

One of the strongest appeals of the initial party identification approach was partisanship's status as both the major long-term component of behavior and, relatedly, a major force in the formation of candidate perceptions and issue opinions, which came later in time and were assumed to be of shorter duration. The Michigan paradigm thus saw partisanship as part of an attitude *system* that had an unambiguous internal causal structure based on the specific temporal characteristics of its component attitudes. Recent research still supports this view, although with some qualifications. Some reciprocal causation between candidate evaluations and partisan identifications has been shown for the short term (e.g., Page and Jones 1979). But other assessments still grant psychological partisanship its traditional role as the principal long-term component of electoral attitude systems (Markus and Converse 1979).

Table 2 contains cross-panel stability coefficients for psychological partisanship, partisan issue opinions, party images, and leader images for each of the three countries in this analysis. As predicted for the United States, partisanship is consistently more stable than attitudes toward other kinds of electoral phenomena. However, opinions on some issues and certain kinds of party images were still fairly durable—perhaps more so than was anticipated in the Michigan approach—even though not as stable as psychological partisanship itself.[13] I shall return to the significance of these data.

Long-Term Partisanship and the Vote in Specific Elections

Traditional political psychology held that long-term, stable partisanship would be an important motivation in people's vote choices in individual elections (as well as a causal influence on short-term attitudes). Since earlier research (Budge, Crewe, and Farlie 1976) had already reported very high correlations between partisanship and the vote in various European countries in synchronic analysis, stable party ties may prove to be a more important factor in European voting than the Michigan approach itself would have forecast. Table 3 shows the results of a test of this proposition. The resulting first-order correlations between stable psychological partisanship measured in two preelection panel waves and the vote in subsequent parliamentary elections in each country were quite high for survey analysis, ranging between .66 and .79 for stable identifications and .59 and .74 for stable past voting.[14] Stable prior party identifications also have a stronger effect on later votes than their U.S. counterpart, thus confirming the importance for the vote of European long-term party ties. Rather than being a meaningless, volatile attitude, as portrayed in some critiques of party identification abroad, European psychological partisanship is for many persons actually a durable attitude with greater consequence for later voting choices than its U.S. counterpart.

Stable Partisanship, Social Cleavages, and Affective Polarization

Cleavage Versus Noncleavage Partisans

Establishing that European party loyalties (and their voting habits variant) are generally quite stable and also have a strong effect on later electoral choices begs the question as to what sustains these loyalties and contributes to their strong effect on subsequent behavior. Since recent research has shown that differences between specific political parties' characteristics influence the nature of psychological party loyalties among those parties' followers, I will look closely at these relationships and other aspects of political context and its effects in this analysis. Elsewhere, large, old parties that have played a role in government have been shown to attract more devoted party supporters than small or new parties with no government experience (Barnes 1990; Cain and Ferejohn 1981; Richardson 1989). Here I am interested in whether a different dimension of party experience (specifically, traditional, cleavage-based party competition and ideological style) contributes to specific kinds of attitude

Table 2. Stable Party Loyalties, Stable Issue Opinions, and Stable Images

Country and Years	Party Loyalties	Issue Opinions	Like-dislike Images	Party Semantic Differential Images	Leader Semantic Differential Images	Party Thermometer Scales	Leader Thermometer Scales	Valence Images	Left-right Self-locations
Britain 1964–70	.76	.30/.41	—	—	—	.27/.28	.25	.40/.56	—
Britain 1974–79	.74	.39/.49	.40/.42	—	—	.63/.64	.48/.49	—	—
Britain 1983–86	.73	.24/.37	—	.22/.54	.10/.37	—	—	—	—
Netherlands 1970–73	.56	.36/.42	—	—	—	—	—	—	.38
Netherlands 1981–82	.71	.46/.60	.58	—	—	—	—	—	.68
Germany 1976	.73	.28/.44	—	—	—	—	—	—	.60
Germany 1986–87	.66	.44/.49	—	—	—	.38/.53	.59/.65	—	.59

Note: Figures are Pearson's correlation coefficients. Issue opinions are based on seven-point scales. Slashes indicate a range of coefficient values across various indicators.

Table 3. Effects of Stable Party Loyalties/Stable Past Voting on Later Vote Choices

Stable Characteristic	Britain 1983–87	Germany 1986–87	Netherlands 1981–82	United States 1972–76
Party loyalties	.79	.72	.66	.51
Past voting	—	.68	.74	—

Note: Figures are Pearson's correlation coefficients for effects of stable psychological partisanship and stable voting (indexes based on earlier-wave patterns) on voting choice in later wave. For specific details of indexes see Appendix.

patterns among partisans in today's electorates. Specifically, I propose that feelings of hostility or estrangement toward opposing parties resulting from over a century of often intense and fundamental social and political opposition will result in heightened interparty hostilities between followers of traditional cleavage parties today and that the resulting interparty attitudes will in turn be an important cause of long-term stability in partisan attachments. I shall further examine a second and related theme to the effect that long periods of ideological conflict between cleavage parties enhances the tendency of cleavage party followers to have well-developed partisan schemata, a factor encouraging both stable partisanship and voting conformity.

We begin with an examination of long-term patterns of party loyalties among followers of cleavage and other parties (Table 4). Partisanship and voting was in fact substantially more stable among supporters of parties associated with traditional major societal cleavages than among followers of noncleavage parties in all three countries.[15] For example, in the Netherlands, Labor party (PVDA), Christian Democratic Appeal (CDA), and Liberal party (VVD) followers in the first panel wave supported, or voted for, the same party in the later wave at levels substantially higher (80% to 85%) than adherents of the main noncleavage party, the Democrats '66.[16] Also, in Germany 72%–75% of Christian Democrats and Socialists voted stably in two elections or supported the same party in two panel waves at levels substantially higher than those for followers of the Green party or the Free Democratic party (FDP). In Britain 81%–83% of Conservatives and Laborites were stable partisans, in contrast with 59% of Liberals and 47% of Social Democrats. Most followers of the parties that had been central actors in traditional sociopolitical cleavage conflict in the respective countries were stable partisans. In contrast, supporters of the remaining parties in these systems displayed less stable attitudes or behavior relative to cleavage party followers.[17] Once these differences are observed, the obvious question is, What can explain them?

Partisan Affect: The Hostility/Estrangement Hypothesis in a European Setting

According to traditional party identification "theory" (Campbell et al. 1960), a positive affective tie was seen as one of the dominant motivations contributing to conforming behavior. In contrast, a few scholars (e.g., Crewe 1976; Goot 1972; Maggioto and Piereson 1977) have proposed that partisanship is more appropriately considered as a choice of a party within the context of the *alternatives* existing within a particular party system at a particular time and that therefore, negative "hostility" to parties other than favored ones may be as important behaviorally as positive ties to liked parties.

Table 4. Stability of Partisanship among Supporters of Traditional Cleavage Parties Compared with Supporters of Other Parties (%)

Political Parties	Respondents Supporting Party at T1	Partisanship				Vote			
		Stable	Latent	Changed	Total	Stable	Abstained	Changed	Total
Netherlands 1981–82	1,095								
Cleavage									
Labor party: PVDA (18%)		81	12	7	100	81	8	11	100
Christian Democratic Appeal: CDA (22%)		82	6	12	100	80	6	14	100
Liberal party: VVD (10%)		88	5	7	100	87	4	9	100
Noncleavage									
Democrats '66 (9%)		25	20	55	100	27	11	62	100
Germany 1986–87	1,311								
Cleavage									
Social Democrtic party: SPD (33%)		75	12	13	100	72	9	19	100
Christian Democratic Union: CDU (33%)		77	12	11	100	75	6	19	100
Noncleavage									
Free Democratic party: FDP (2%)		38	21	41	100	43	8	49	100
Green party (4%)		47	28	25	100	56	9	36	100
Britain 1983–87	869								
Cleavage									
Labour party (24%)		83	6	11	100	—	—	—	—
Conservative party (40%)		81	9	10	100	—	—	—	—
Noncleavage									
Liberal party (15%)		59	12	29	100	—	—	—	—
Social Democratic party (7%)		47	10	43	100	—	—	—	—

Note: Figures are percentages of respondents who supported or voted for a particular party according to first-wave answers who supported the same party in later-wave panels. Figures in parentheses are percentages of all respondents (at T1) who supported a specific party and indicate, therefore, general importance of party in respective system. "Latents" are persons who supported a party in first wave but said *no party* or *don't know* in second wave.

Table 5. Average Out-Party Hostility

Country and Year	Cleavage→Cleavage	Cleavage→Noncleavage	Noncleavage→Cleavage
United States 1984	45	—	—
Britain 1983–86	83	33	58
Netherlands 1981–82	83	64	41
Germany 1986–87	45	54	35

Note: Cleavage→Cleavage refers to (average) percent of negative thermometer-type ratings of alternative cleavage parties (or negative out-party feeling/reaction based on alternative question formats) among stable supporters of cleavage parties. Cleavage→Noncleavage are ratings of noncleavage parties by cleavage party supporters. Noncleavage→Cleavage is the reverse of the above.

As we suggested already, one of the plausible legacies of traditional sociopolitical cleavage-based conflict is hostility or estrangement between adherents of opposing cleavage parties. Cleavages are by definition profound divides between persons of different religious, class, or ethnolinguistic backgrounds (Lipset and Rokkan 1967). Since vestiges of these social divides are still found today in the structural alignments of social groups (Heath, Jowell, and Curtice 1985; Klingemann 1984) and also in the manifestos of traditional cleavage parties, interparty hostility may be a common component of European partisanship alongside positive feelings toward preferred parties. Answers to questions that solicited evaluations of nonsupported, as well as supported, parties were used in each of the three countries to test this "party hostility/estrangement" hypothesis. Table 5 shows averaged figures for out-party negative feelings among aggregate groups of cleavage and noncleavage party supporters. United States findings based on the 1984 National Election Survey are included for purpose of comparison.[18]

In two out of three European countries—Britain and the Netherlands—feelings of estrangement from other cleavage parties were much stronger among cleavage party followers than among noncleavage party adherents. Cleavage party followers in these two countries also had much stronger feelings of out-party estrangement than U.S. Democrats and Republicans (the traditional benchmark groups where party loyalties are concerned). Even though most research suggests we have entered the twilight age of traditional cleavages, a cleavage-based pattern of interparty emotional feeling still prevails in the minds of voters who support older cleavage parties in two out of the three European countries in this analysis.[19] Only in Germany were the interparty affective attitudes of supporters of cleavage parties more moderate and like those in the United States. German partisans and supporters of noncleavage parties manifest a softer, more open partisanship. In the German case, both the softening of traditional religious cleavages through the inclusive appeals of the CDU and the moderating of the SPD's policies after the famous Bad Godesberg conference of 1959 are relevant to the softening of traditional cleavage feelings.[20]

Party Distance

The aggregate patterns of affect just reviewed provide useful baseline information on interparty attitudes among partisan groups in each of the three European countries. At the same time, the aggregate figures may not accurately reflect individual level feelings. In order to understand individual level differences in relative ratings of different parties and their effects, I calculated a party distance score

Table 6. Feelings of Party Distance (%)

Country and Year	Supporters of Working-Class Parties	Supporters of Middle-Class/Christian Parties
Britain 1986	94	94
Netherlands 1982	83	71
Germany 1986	67	52

Note: Figures are percentages of stable party followers (based on answers to traditional party loyalties questions) whose relative sense of attraction to supported parties exceeds averaged evaluation of nonsupported parties.

for each individual based on averaged affective feelings (positive or negative) toward nonsupported parties subtracted from affective feelings toward supported parties. Subsequent analysis using this index indicated dramatically polarized attitudes among cleavage party supporters in Britain, moderately polarized feelings among supporters of old cleavage parties in the Netherlands, and only weakly polarized feelings among supporters of traditional cleavage parties in Germany (Table 6). The effects of traditional cleavage conflict are obviously present but with some important variations between countries.

The correlations between individuals' net party distance feelings and stable party loyalties, stable partisan voting, and also vote choices in later parliamentary elections in each country are shown in Table 7. In all three countries, both stable psychological partisanship and stable voting records were strongly affected by feelings of party distance.[21] Party distance sentiments also had a strong effect on voting in later elections in all three countries. Feelings of party distance influence the vote much like the stable loyalties and voting variables examined earlier. This fact—plus the strong direct relationships between party distance feelings and stable loyalties and stable partisan voting—indicates that out-party feelings are an important component of the behavior of followers of old cleavage parties.

Ideology and Imagery: The Cognitive Component of Cleavage Politics

Long-term adherence to specific political principles by cleavage-linked parties (Heath, Jowell, and Curtice 1985; Rose and McAllister 1986) could also sustain stable partisan attachments among those parties' followers by providing ideological anchoring to partisan loyalties. If a durable echo of traditional cleavage politics is

Table 7. Relationships between Party Distance Feelings and Stable Party Loyalties, Stable Voting and Vote Choice in Later Parliamentary Elections

Country and Year	Distance-Stable Party Loyalties	Distance-Stable Voting	Distance-Vote in Latest Election
Britain 1983-86	.82	.54	.77
Netherlands 1981-82	.61	.65	.77
Germany 1986-87	.51	.64	.54

Note: Figures are Pearson's correlation coefficients.

Table 8. Stable "Issue Opinions" and Imagery as Overlapping Components of Stable Partisan Schemata among Supporters of Traditional Cleavage Parties (%)

Political Parties	Both	Either	Neither	Total
Britain 1983–87				
Working class (Labour party)	59	37	4	100
Middle class (Conservative party)	53	41	6	100
Middle class/religious	—	—	—	—
Netherlands 1981–82				
Working class (Labor party: PVDA)	61	33	6	100
Middle class (Liberal party: VVD)	26	55	19	100
Middle class/religious (Christian Democratic Appeal: CDA)	16	49	35	100
Germany 1986–87				
Working class (Social Democratic party: SPD)	38	45	17	100
Middle class	—	—	—	—
Middle class/religious (Christian Democratic Union: CDU/Christian Socialist Union: CSU)	40	39	21	100

Note: Figures are simple percentages of stable partisans in each category. Missing data are not reported. *Both* refers to persons with stable party ties combined with both congenial stable opinions on party principles and stable images; *either* refers to persons with party ties and either congenial stable issue opinions or stable images; and *neither* refers to persons with stable party ties and neither congenial stable issue opinions nor stable images.

to be found among today's supporters of European parties, a significant number of ordinary citizens should manifest stable opinions on long-term party principles and party images. Our findings shown earlier (Table 2) demonstrate that this condition is met in all three electorates. Cross-panel stability coefficients for long-term issue opinions and images ranging as high as .6, with means in the vicinity of .3 and .4, were found in all three countries. While fewer people have stable opinions on party principles and stable images than party loyalties, stable opinions on basic principles and stable party images are still not uncommon in European electorates. For quite a few people issue opinions and other attitudes are of equivalent durability to partisanship itself.

Since there are sizable groups of persons whose issue opinions and images are as stable or nearly as stable as partisanship itself, we can anticipate *overlapping combinations* of mutually congenial stable imagery, stable views on party principles, and stable partisanship among quite a few persons. Some persons will have fully developed partisan schemata wherein affective loyalties are buttressed by both stable images and stable issue views. Other partisans may have less developed schemata consisting of only favorable images of their preferred parties or durable opinions on party principles congenial to their loyalties. There may also even be some persons whose partisanship lacks any form of cognitive buttressing.

As Table 8 demonstrates, there were both sizable intercountry and interparty followership differences in proportions of stable partisans who actually had fully developed partisan schemata.[22] The observable patterns partially supported the hypothesis that supporters of cleavage parties should have well-developed partisan schemata. Majorities of stable supporters of three traditional cleavage parties—Britain's Conservative and Labor parties and the Netherlands' Labor party—did combine stable party ties with both stable and congenial images and issue views. These persons' durable party loyalties were thus buttressed by extensive supportive schema-stored beliefs, especially among supporters of the two working-class parties. Supporters of working class parties in Britain and the Netherlands were actually twice as likely to have high levels of stable overlapping partisanship and opinions on basic principles as were supporters of middle-class or religious cleavage parties. This could represent the effects of more explicit and consistent ideological cues from party leaders or a tendency observable for majorities in both the Dutch and British electorates to favor left-wing or secular positions on issues relating to both traditional social welfare state programs and some new issues including the environment.[23] The reduction of the Labor party in Britain to its hard core of support after the 1979 election might also be reflected in high levels of stable, confirming ideological partisanship among its supporters. Whatever the cause, the tendency of stable supporters of working-class parties to have issue-opinion-buttressed attitude schemata was unambiguous in two out of the three countries.

However, stable partisans favoring the remaining four cleavage parties—the Dutch CDA, Dutch Liberals, German Social Democrats, and German Christian Democrats—tended more to have *either* congenial positions on party principles or supportive images, but not both schema components. Within this second grouping, there were further differences between countries. First, reflecting the decline in conflict over traditional cleavage issues in their country (Baker, Dalton, and Hildebrandt 1981), German cleavage partisans had less-developed schemata than British partisans and Dutch PVDA supporters, regardless of which party was supported. Followers of the two German parties had either congenial stable images or reinforcing stable issue views in approximately the same proportions. In contrast, supporters of the Dutch CDA and Liberal parties tended to have only stable congenial images. In contrast to the more overtly issue-laden nature of stable left-wing party support, Dutch religious party supporters tended more commonly to be stable image partisans. Stable Dutch CDA supporters also lacked reinforcing cognitive attitudes more than partisans of any other major cleavage party. For one out of four stable CDA adherents, a durable party attachment lacked cognitive anchoring and was accompanied only by a proclivity to vote stably.[24] However, supporters of Germany's "religious," middle-class party, the CDU, were almost identical in their levels of issue partisanship to SPD followers.[25] Both national and specific party considerations were important.

Differences in stable partisanship and evaluative buttressing reflect specific party strategies, the effects of time, and specific system parameters and alternatives. As anticipated by the schema approach, high concentrations of supportive issue opinions and imagery are found to supplement stable partisanship so as to encourage consistent voting (Table 9). People whose stable party ties were complemented by both supportive stable images and stable issue opinions were more likely to vote for their preferred party in all three countries relative to persons having less-developed schemata. Other combinations of stable psychological components and stable partisanship had predictably weaker effects on voting. Stable partisans with *either* solely congenial imagery or congenial policy views were less likely to vote consistently compared with the stable partisans having the fullest possible overlap of reinforcing information and beliefs. Likewise, persons whose stable partisanship was not buttressed by either congenial imagery or supportive issue opinions were least likely to vote for

Table 9. Effects on Vote of Stable Party Loyalties Combined with Overlapping Stable Issues and Images

Country	Both	Either[a]	Neither
Britain	.62	.31/.30	.11
Netherlands	.44	.31/.22	.22
Germany	.48	.35/.33	.22

Note: Figures are Pearson's correlation coefficients.
[a]First figure pertains to issue opinions and second figure to party images.

their preferred party in a later election.[26]

Table 10 carries the analysis of the effects of partisan schemata one step further by contrasting the contributions of affect with those of the different schema categories. In so doing, the contribution of cognitive aspects of partisanship relative to those of the traditionally affective components are tested. Affective ties are represented by a "distance" measure calculated by subtracting people's feelings toward their preferred party from their averaged feelings toward nonpreferred parties, (see, again, Table 6). As before, all measurements are based on prior wave responses and are used to see how prior commitments affect later-wave voting choices. In two countries the cognitive elements of partisanship were collectively more important than affective distance feelings. For voters in the Netherlands, affect was the primary driving force, in large part due to the absence of congenial cognitive partisanship among CDA supporters.

The Meaning of Stable Voting in Germany and the Netherlands

One of the puzzling features of the analysis has been the discovery in two countries of a sizable group of stable voters who do not report stable party loyalties in replies to the traditional partisanship questions. How can people who vote consistently not answer the partisanship questions properly? Are these partisans conceptually different from psychological partisans? If so, how? These questions invite a full-scale treatment beyond the scope of this research. However, initial findings (Table 11) indicate that these habitual voters in both Germany and the Netherlands were, up to a point, much like their counterparts, the stable psychological partisans. Both groups have stable issue opinions and party images that are congenial to their stable loyalties or stable voting. In Germany habitual voters were not actually much different from stable partisans. In contrast, in the Netherlands, feelings of party distance play a bigger role among habitual voters as compared with stable psychological partisans. Dutch habitual voters are to some extent cognitive partisans; but they are even more likely to be persons with feelings of out-party hostility.

Obviously, the traditional party identification and party proximity questions do not capture all party supporters. Stable voting is a variant of stable partisanship. In some instances it is driven mainly by negative feelings toward other parties.

Table 10. Effects on Vote of Affective Distance Versus Cognitive Beliefs

Attitude	Britain	Netherlands	Germany
Distance	.49	.65	.32
Images and issue opinions	.30	.17	.33
Party images alone	.12	.07	.25
Issue opinions alone	.12	.13	.22

Note: Figures are standardized beta coefficients.

Table 11. Stable Party Loyalties Versus Stable Voting

Stable Characteristic	Netherlands 1981-82	Germany 1986-87
Partisanship		
Stable issue opinions	.45	.50
Stable images	.60	.71
Party distance feelings	.86	.74
Voting		
Stable issue opinions	.23	.44
Stable images	.46	.63
Party distance feelings	.73	.66

Note: Figures are first-order correlations. Missing data were excluded from stable partisanship and stable voting.

Future research should try to identify the sentiments of these negative partisans more effectively via measurements that focus on feelings toward alternative parties as well as those that tap positive affect for a preferred party. These stable voters are obviously conceptually important, and their proportions within the German and Dutch electorates relative to those of stable psychological partisans identified using conventional party identification questions are significant (see also Miyake 1991 and Richardson 1986 for a discussion of a related phenomenon in Japan).

Conclusion

Analysis of survey findings from Britain, West Germany, and the Netherlands in the 1980s has shown that majorities of persons in each country have stable party ties and/or stable voting histories. Many persons—between 50% and 70% of the persons interviewed in each country in the 1980s—were thus stable psychological partisans. In addition, a significant group of citizens in West Germany and the Netherlands were stable voters but not stable partisans. The same propensities to stability in party loyalties and/or voting were present among persons in the 1960s and 1970s, despite considerable electoral turbulence and social change during those times. Even granting slight distortions favoring stable patterns due to employment of panel-based findings, the quotient of stability in individual-level partisanship and voting is impressive. Its importance is further enhanced by a tendency for earlier stable psychological partisanship to be a dominant influence on later voting choices in each country.

Sensitive to the implications of political context, I found that followers of old cleavage parties in all three countries were much more prone to stability than supporters of other parties. The obvious question emerges as to what motivates the high proportion of voting choices that are consistent with prior stable partisanship and the higher frequencies of these kinds of voters among followers of old cleavage parties. The analysis indicated that the emotional and ideational residue of traditional cleavages and of intracleavage party politics drives stable party ties and voting in the three Northern European settings I examined. Comparatively high out-party hostilities/estrangement among supporters of traditional cleavage parties in Britain and the Netherlands was one important finding. Party distance scores incorporating the twin feelings of attraction to favored parties and hostility toward, or feelings of estrangement from, other cleavage parties in each system were virtually substitutable for psychological loyalties in regression analyses. Among supporters of old cleavage parties' followers, only those in Germany showed a limited tendency toward strong estrangement from opposing parties, most likely as a result of the moderation of electoral politics in that country since the 1950s.

Further analysis indicated that interlocking affective-cognitive schemata (including the just-mentioned party distance feelings) among supporters of old cleavage parties were the main factor contributing to individual-level inertia in psychological responses to politics. For many British and Dutch citizens stable partisanship was also a package of interrelated cognitive, evaluative and affective elements. In contrast, supporters of Germany's two traditional cleavage-linked parties—the Christian Democratic/Christian Socialist Union and German Socialist party—were themselves mainly stable cognitive (issue or image) partisans, since their out-party hostility levels were much lower than those of their counterparts in the Netherlands and Great Britain. Overall, stable partisanship demonstrated a "cleavage echo" in all three countries even though the effects of traditional sociopolitical conflict were most visible in Britain and the Netherlands. These well-developed and stable schemata of followers of old cleavage parties also underwrote the substantial consistency I discovered between earlier stable party ties and later voting. Even new environmental and social issues have been integrated into the more traditional patterns of alignment in ways that enhance rather than undermine stable voting. Social and political change has not entirely erased the contribution of traditional cleavage styles of politics.

In Further Defense of the Schema Metaphor

As is well known, traditional political psychology (Campbell, et al. 1960) saw psychological partisanship as an independent causal force in an attitude system where issue opinions, candidate evaluations, and other short-term attitudes were structurally dependent on partisanship. Contributing to partisanship's independent causal status was its assumed primacy, or the expectation that it is learned earlier in people's lives than other attitudes. Also favoring partisanship's dominance was the view that as a reflection of the greater durability of party objects, people's party ties were more stable over time than other kinds of electoral attitudes.

Recent research, most notably Fiorina's (1981) view of retrospective partisanship and voting, has qualified the traditional view of interattitude relationships by explicitly challenging the traditional assumption of affective partisanship's primacy. Affective partisanship's dominance and primacy can also be qualified as the result of our own European evidence. Quite a few people have been shown to have stable issue opinions *or* party images, or both kinds of stable attitudes, over the same time periods in which stable party loyalties are identified. Since we can also show that these other kinds of stable attitudes measured in the same panel waves complement stable party loyalties for many persons, the primacy of affective loyalties' cannot really be asserted within the contours of the panel evidence examined.[27] If it cannot be established that affective party loyalties come first, it follows that the affective components of partisanship are not necessarily the dominant components of the schema structures. Seeing stable partisanship as a schema incorporating different interrelated, temporally simultaneous (but also long-term) affective and cognitive/ideational elements for substantial sectors of the electorates thus makes sense.

Is European Partisanship a Partisan Identification?

One question remains unanswered, yet is central to an evaluation of the role of stable partisanship in European voting: What is the relationship of stable European partisanship to the U.S. party identification concept? Is stable partisanship an identification? The answer is *yes* if we consider evidence indicating generally high levels of liking for one's own party in each country (which were also comparable to those in the United States in general, and higher than the U.S. example in the case of cleavage party followers).[28] Many traditional cleavage party supporters also strongly rejected, or at least felt estranged from, alternative cleavage parties. Since a negative out-party feeling is also logically the outcome of a strong reference group attachment or identification (or perhaps vice versa), the stable partisanship among supporters of traditional cleavage parties (especially those in Britain and the Netherlands) would seem actually to have enhanced identificational properties relative to its U.S. counterpart.[29]

However, European stable partisanship has a much stronger effect on later voting choices than was forecast by the Michigan school in the pilot formulation of the party identification concept.[30] Nor was the tendency toward habitual voting pointed out earlier by Lawrence Le Duc (1981) and nearly a decade ago by Thomassen (1976) expected. So we have a European partisanship with stronger identificational properties where people are cleavage partisans, yet its strength makes it a much stronger influence on later voting. The party identification concept really doesn't fit as it was initially formulated.[31]

My analysis also indicated that long-term cognitive aspects of partisanship—images of parties, support for party principles, and overlapping combinations of these two elements—were collectively more important than partisan affect (measured as distance between preferred party strength of attachment and evaluations of alternative parties) in two of the three countries examined. European partisanship in the 1980s is cognitive partisanship to a substantial degree.

It would be nice to know the extent to which stable schema partisanship reflects enhanced cognitive competence resulting from social change (Dalton 1984; Heath and McDonald 1988) or greater information exposure resulting from television's enormous importance in recent years. We should also watch closely to see whether cognitive partisanship is ultimately more vulnerable to changing political environments than the more "primitive" affect-based partisanship envisioned by the Michigan school (and probably also implicitly by political sociology). If it is, the implications for systemic stability are obvious.

I have identified and analyzed an important element of partisan stability in European electorates and demonstrated that stable partisanship is the dominant component in subsequent voting choices in three European electoral settings. I do not rule out change; to do so would be inaccurate in light of recent episodes of electoral volatility in all three countries. What I do suggest is that change and volatility have occurred in a much more inert attitudinal environment than has been thought to exist. Further exploration of the relationship of this inertia to change, where change has occurred, must be the subject of future research.

Appendix

Characteristics of Data Sets

Country and Period	N	Wave 1	Wave 2	Wave 3	Wave 4
Britain 1983–87	703	Jun 83	Jul 86	Jun 87	—
Britain 1974–79	381	Feb 74	Oct 74	May 79	—
Britain 1963–70	936	1963	1964	1966	1970
Germany 1986–87	1,954	Sep 86	Jan 87	Feb 87	—
Germany 1976	1,196	Mar–Jun 76	Aug–Sep 76	Oct–Nov 76	—
Germany 1974–80	912	Jan–May 74	Mar–Jul 80	—	—
Netherlands 1981–82	1,095	Jan–Feb 91	Apr–May 81	May–Jun 81	Sep 82
Netherlands 1970–72	972	Mar–Jul 70	Apr 71	Nov 72	—

Stability Coefficients and Indexes

Britain 1983–87	All measures use wave 1 and wave 2 responses.
Britain 1974–79	All measures are based on wave 1 and wave 2 responses.
Britain 1963–70	All measures are based on wave 2 and wave 4 responses.
Germany 1986–87	All measures except images used wave 1 and wave 2 responses; image index is based on wave 1 and wave 3 responses.
Germany 1976	All measures are based on wave 1 and wave 2 responses.
Netherlands 1981–82	Partisanship, like-dislike images use wave 1 and wave 3 responses; left-right self-locations, issue opinions use wave 2 and wave 4 responses.
Netherlands 1970–72	All measures are based on wave 1 and wave 2 responses.

Index Construction

Partisanship. Index is based on responses to two panel waves, typically waves 1 and 2. Respondents indicating same party on both waves are assigned a code for that party. Persons supporting a party at one point in time and *no party* or *lack of knowledge* at another time were coded as missing data. Persons who changed were also coded as missing data, as were consistent *don't knows, not applicables* and abstentions.

Voting. The index of prior stable voting employed in analysis of the Dutch data was based on recall reports of people's votes in an earlier election combined with reported votes in the 1981 election. This index was used in subsequent analysis of the 1982 vote. In West Germany, since the German panel only covered one election period, recall of past vote was used along with first-wave voting intentions to construct a habitual voting index. (Use of recall data leads to a slight bias in favor of consistency, as I have shown elsewhere [1988].)

Images. Image indexes were based on answers to like-dislike questions in waves 1 and 3 in the Netherlands 1981–82 study, semantic differential image questions from waves 1 and 2 in Britain 1983–87, and like-dislike questions in an early wave and semantic differential questions in a later wave in Germany 1986–87. Party and leader images were measured as semantic differentials (British Election Study [BES] 1983–87, BES 1974–79, German Election Study [GES] 1987) and as thermometer/skalometer dimensions (Netherlands Election Study [NES] 1981–82, GES 1987). Traditional like-dislike measures of party images are found in some (BES 1974–79, NES 1981–82, BES 1963–70, NES 1970–73) but not all studies.

Issue Opinions. Stable issue opinions indexes were constructed using answers in two interview waves to questions that had the same format. A variety of different measures were used to measure issue attitudes, including seven-point position scales (BES 1983–87, NES 1981–82, GES 1987), four-point position scales (all three British studies), *gone-too-far/about right/not far enough* trichotomies (BES 1983–87), *agree-disagree* and *should-should not* dichotomies (BES 1983–87), and party competence scales (GES 1976). Self-locations on seven-point scales from the 1980s studies are the source of issue opinion data employed in this study.

Affective Distance Measures

An index of party distance was constructed in each country using averaged ratings of alternative parties subtracted from ratings of one's own most positively evaluated party. The U.S. questions employed a 100-degree *like-dislike* thermometer, the German variant was an 11-position "skalometer," the British findings were used in a 7-position index based on separate measures of *attraction-nonattraction* and the relevant Dutch question asked people their likelihood of voting or not voting for a specific party on a 10-point scale.

Panel Interval/Duration

The party (and other attitude) stability figures I cite in the text are based on responses to panel waves separated by very different amounts of time. The German panel intervals were very short in duration and lasted only a few months. The Dutch survey panel intervals were somewhat longer. The British studies' panel intervals were longest, being three years in the case of the 1983–87 survey. However, differences in time intervals did not result in enormously distorted and nonequivalent findings.

Where effects of different panel interval lengths could be examined, as in the 1963–70 BES, there was only modest decrease in cross-panel stability figures as time periods became longer or more elections were included. Eighty percent of the respondents interviewed after the 1964 election in Britain still supported the same party after the 1966 election, an interval of two years and one election. A drop of only 5% to 75% who were stable partisans could be seen in the longer 1964–70 interval of over four years and two elections. So the differences attributable to differences in time intervals were probably not so great after all.

Obviously, I could not be certain to what extent the panel technique's well-known bias toward stability underlay the patterns I have described. Since perfect comparability was not possible, while differences in length of interval effects were not enormous, I looked at the very longest possible periods to arrive at maximum term stability figures within the limits of each of the data collections.

Parental Influence

Among persons interviewed in major election studies or the Political Action Study in the 1960s and 1970s, the percentages of total populations who knew their father's partisanship and conformed to that partisanship (if their father was a partisan) were as follows:

Australia 1967	48
Austria 1974	28
Britain 1963	44
Britain 1974	42
Canada 1965	33
Finland 1975	27
Germany 1973	26
Germany 1974	18
Italy 1975/76	18
Japan 1967	20
Norway 1965	35
Netherlands 1970	24
Netherlands 1974	30

Notes

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1. According to Shamir (1984), voting in recent decades is no different from earlier eras, in the sense that some volatility is normal amid general stability.

2. Use of the schema approach reflects my search for a social-psychological concept that would fit the combinations of affective and cognitive feelings I could see in my findings. The schema concept has traditionally referred to hierarchical structures of information stored for later retrieval under category labels. This filing cabinet approach to schema has been expanded to include causal structures, which is one of these senses I employ in this manuscript. Recent work by Nisbet and Ross (1980), who see people as reacting to the world as intuitive scientists, that is, gatherers of information and testers of simple hypotheses as they seek causes of events and phenomena and estimate consequences of their own behavior, was instructive on this point. In addition, some recent research has been schemata as carrying imbedded affective tags (see Fiske 1982).

3. However, while the reciprocal causation studies focused on attitudinal interdependence in the short term, my own findings locate nodes of interdependency in the longer term of survey panel intervals.

4. Enhanced cognitive capability that reflects increased educational attainment (Dalton 1984) might also make the cognitive accompaniment to affective partisanship more central and substantial than in the past. Earlier research asserting that party images, which are cognitive beliefs about parties' "essential qualities," are both widespread and contribute as much to voting choices as affective identification (Matthews and Prothro 1966; see also Butler and Stokes 1969) also implies an important role for nonaffective aspects of party loyalties.

5. Political party manifestos still echo older class or religious ideological debates. See, for example, the manifestos of the British Labor and Conservative parties at the time of the 1987 election, as excerpted in *Keesing's Contemporary Archives* (Vol. 33, July 1987, Item No. 32256).

6. Images of parties can directly reflect class ideological competition when couched in terms of cleavage interests and their representation, as is the case in certain British Election Study party image questions. Images of party competence can also be based on performance relative to ideological goals of class or other cleavage representation.

7. The role of affect in the schema approach is quite different from that in traditional partisanship theory. People's affective sentiments may be stored along with relevant cognitive information and beliefs and retrieved along with other related information in response to the appropriate stimuli (Fiske 1982). However, affective feelings in this instance can result from internal argumentation based on knowledge processing and evaluation, rather than the other way around, the traditional view. In other words, cognition could precede affect. For many Europeans, cognitions may generate affective loyalties. Although affect may precede favorable cognitions and evaluations (Zajonc 1980) in some cases, the reverse is likely very common.

8. The characteristics of the different data sets and related aspects of index construction are discussed in the Appendix.

9. Obviously, there were still some differences between the three countries. While the Netherlands had the same number of important parties as the other two countries, there were still many more minor parties there than in Britain or Germany.

10. There were also some other differences between the three countries that could be relevant to my analysis, though the linkages are not really clear. For example, the Liberals occupied a slightly different place in the British system as compared to the place of minor parties in Germany and the Netherlands. The presence of a single-member district system with plurality rule in Britain has often been cited (e.g., Butler and Stokes 1969) as potentially producing a weaker voting commitment among that party's partisans. Up until 1974, the absence or weak position of Liberal candidates in many districts meant that Liberal supporters could either waste their vote or vote strategically for another party. Neither of these situations could be expected to enhance party ties. In contrast, supporters of minor parties in the two continental systems could feel that their vote for their preferred party was meaningful within the context of proportional rules for allocation of seats. Opportunities for coalition participation were also different for minor parties in the different systems. The German Free Democrats were frequent coalition partners in alliances with the CDU or SPD and might be seen by supporters as relatively credible for this reason. The Democrats '66 in the Netherlands did not have the same kind of credibility based on past coalition participation as the FDP. But they could potentially be a coalition partner, given some variability in Dutch party coalitions over time. The British Liberals lacked such credibility in the periods under review. I further clarify the credibility question and examine its effects on party loyalties elsewhere (Richardson 1989).

11. If only persons who actually voted in both of two separate elections were to be considered, three persons out of every four were consistent.

12. Analyses of data from the Butler and Stokes (1969) long-term panel from 1960s Britain, the 1974–79 British Election Study panel, the 1970–72 Dutch Election Study panel and the 1976 German Election Study panel showed virtually identical patterns of general partisanship stability to those indicated above in Table 1. In addition, the relative

partisanship-stable voting differences reported for the 1980s were also confirmed for earlier eras (see also Le Duc 1981). Even though the 1970s were a period of volatility in the vote in Britain and elsewhere, large and significant proportions of each country's enfranchised citizens were still stable partisans or stable party voters in the periods covered by panel surveys.

13. Two other attitudes toward electoral objects showed stability levels close to those reported in Table 3 for partisanship. One was left-right self-locations in the Netherlands in the 1980s (see also van der Eijk and Niemoller 1983). However, other cross-panel findings on left-right self-locations were less stable than partisanship, both in the Netherlands (1970-73) and in Germany in the 1976 and the 1987 election survey data. The second attitude whose stability matched party loyalties or voting was thermometer ratings of Germany's Chancellor Kohl in 1986-87. This figure was also an outlier relative to other cross-panel measures of leader evaluations. For example, British panel survey findings suggest that the stability of leader evaluations usually deteriorates much more rapidly than party loyalties as time intervals between panels get longer. However, given recently increased interest in leader evaluations (Bean and Mughan 1989; Graetz and McAllister 1985), the long-term properties of leader attitudes and their relationship to other kinds of electoral attitudes obviously need closer examination.

14. To test this proposition, stable partisanship indexes were constructed using responses in two earlier-wave interviews and then correlated with actual partisan voting choices reported in later-wave interviews. (Since the vote was more stable over time than self-reported psychological ties to parties in both the Netherlands and Germany, stable voting indexes were also constructed for those two countries, in addition to stable partisanship indexes.)

15. Cleavage parties are identified as the major post-World War II representatives for long-standing class or religious divides. This category includes the British Conservative and Labor parties; the Dutch Labor, Liberal, and Christian Democratic parties; and the German Christian Democratic and Socialist parties.

16. Note that we treat the CDA as a cleavage party even though this party is more properly a cleavage successor party. The fact that its supporters display characteristics appropriate to the cleavage type is interesting on its own grounds. The CDA has obviously mobilized followers with some of the characteristics of the former supporters of the Catholic People's Party, CHU, and Anti-Revolutionary Party, who always distanced themselves from the PVDA in answers to thermometer questions (Richardson 1989).

17. If followers of the politically important, large, old cleavage parties constituted the total electorates in the respective countries, general levels of partisan stability would obviously be even higher than they are where calculations are based on total populations (see also Cain and Ferejohn 1981; Richardson 1989). This important feature of European behavior is often overlooked in voting analysis.

18. Different questions were used in the different country surveys. See the Appendix for details. The Dutch study item was furthest removed from the concept of positive affect-hostility, in that it sought people's self-estimates of their future voting tendencies in regard to each major and most minor parties. The results were clearly not indicators of hostility or liking, at the same time that they accorded nicely with thermometer-based information from earlier surveys.

19. The interparty distance findings on Britain could be said to reflect the polarization seen in British politics in the 1980s. To some extent they did so. The out-party dislikes were about 10% higher among both Labor and Conservative party supporters in the 1980s than was the case a decade earlier. At the same time, British "out-party" feelings even in the 1970s were much more commonly negative than in the case of the United States.

20. In the German case, there were intensely negative feelings on the part of the Greens toward the Christian Democrats and Free Democrats. The sharpness of intergenerational differences and conflict over the environment is reflected in the Greens' disdain for other groups. Multiple axes of affective feelings thus exist, depending on the nature of competition in each party system. Analysis of responses to *like-dislike* questions using the Dutch and German data sets showed identical patterns to those based on thermometer questions or other similar measures reported in Table 5, with the exception that there were larger numbers of persons who did not answer these questions.

21. The effects of feelings of party distance were actually greater on stable voting habits in both the Netherlands and West Germany than on stable partisanship. Even though the effects of past cleavages are most muted at the psychological level in West Germany, they still help explain the stable voting phenomenon there, as well as playing a similar role in the Netherlands.

22. Schemata patterns were determined by looking at patterns of overlap/consistency between party loyalties, a party image index, and a party principles index.

23. The attraction of persons in "new" middle-class categories to left-wing parties and left-wing positions on environmental and other "postindustrial" issues has been widely noted (e.g., Baker, Dalton, and Hildebrandt 1981). Where such a pattern prevailed, it could have had an additional enhancing effect on the schemata of working-class parties' supporters, since these persons could be assumed to have higher cognitive competence than

traditional blue-collar supporters of these parties (Dalton 1984).

24. Looking at matters slightly differently, the cumulative ideational or cognitive component of stable partisanship (the *both* and *either* categories combined) proved to be quite high for supporters of the two British parties (95% for Labor and 94% for the Conservatives) and for Dutch PVDA partisans (94%). Stable cognitive beliefs or images supportive of party loyalties were also shown by more than 80% of the stable cleavage partisans in Germany and by Dutch Liberals. For Dutch CDA followers, cognitive buttressing of one or another form was found in just 75% of the cases.

25. To the extent that we could trace their issue opinions meaningfully, noncleavage partisans in Britain and Germany demonstrated a high incidence of issue partisanship. In the case of the FDP and the Democrats '66, supporters combined stable policy commitments with low affect toward both their own and other parties. The German Greens were somewhat different, given their combination of strong stable issue orientations with a high degree of outparty hostility. The attitudes of stable British Liberals/Social Democrats were so complex as to defy easy classification. As has already been shown elsewhere (Heath, Jowell, and Curtice 1985), Alliance supporters favored left-wing positions on some issues, conservative issues elsewhere, and "liberal" positions on issues like the death penalty.

26. In addition to showing the effects of different combinations of stable party loyalties, stable issue views, and stable party images, the Table 9 findings serve as a validity check on my methods. There were nearly three times as many issue items in the British 1983-87 panel survey as in the Dutch 1981-82 and German 1986-87 studies. The different studies also incorporated different party image items and combinations of items. The convergence of the findings for all three countries in regard to the relationships between the *both*, *either*, and *neither* categories and conforming votes was heartening.

27. Although not my focus here, the European evidence for partisanship's primacy in the form of documented parental influence on partisanship is also fairly weak. In the Appendix I report findings on intergenerational conformity in 11 countries outside of the United States, using 1960s and 1970s recall data. Only in Britain and Australia were there indications of parental effects similar to those in the United States. In 7 countries the percentages of respondents who reported loyalties that conformed to their memories of parental preferences were roughly one-half of the U.S. levels at the same general point in time. These earlier findings from the 1960s and 1970s should actually display parental influences at a maximum advantage (with the exception of Germany, Japan, and Italy, where systemic disruptions had special effects on partisanship) relative to more recent studies, which show a trend of decline since that time (Rose and McAllister 1986), given the expected effects of social change. For that reason, the evidence of fairly meager parental influence is significant. There is ample doubt as to both the absolute and relative (compared to the United States) importance of parental partisanship abroad (cf. Cameron 1972/73; Converse and Dupeux 1962; Percheron and Jennings 1981; Rose and McAllister 1986).

28. The data are not shown. But among followers of 12 parties in three countries, only supporters of Britain's Liberals and Social Democrats were conspicuously weak affective partisans in the sense that less than 80% of followers actually liked their supported party.

29. The strong element of mutual estrangement between supporters of the Dutch CDA, a party formed only in 1980 as a successor to three Christian cleavage parties, and followers of the Dutch Labor party (PVDA), also reinforces my general notion that the styles of cleavage politics continue to persist even when the initial features of cleavage systems have changed.

30. My use of a stable partisanship index based on *early*-panel-wave measurements in an analysis of *later*-wave voting choices helps minimize problems associated with contemporaneous attitude and behavior measurement cited in earlier critiques of party identification (e.g., Budge, Crewe, and Farlie 1976). Voter confusion of their vote intention and psychological partisanship in replies to survey questions is simply not as severe a problem when partisanship is measured at earlier points in time from the vote, including at least one measurement from a much earlier date. I thus avoid a major weakness of earlier European party identification research while demonstrating the importance of durable psychological partisanship. An additional reason for the strength of the partisanship-vote relationship may be the higher salience of parties I identified in the three European countries. Thirty-four percent of U.S. citizens in 1984 reported no likes or dislikes of a party, whereas the comparable figures were 3% for Germany (1986), 7% for Britain (1974) and 9% for the Netherlands (1982). (Standardization of these figures to accommodate differences in numbers of parties slightly enhanced the proportions of zero answers in the three European cases but did not affect relative tendencies). These differences presumably reflect the greater importance of parties in parliamentary systems, and in countries with proportional representation–party list systems or where party organization has traditionally been more penetrative than the U.S. case.

31. Actually, the U.S. pattern is the deviant one, as at least one study (Le Duc 1981) has already pointed out.

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THE POLITICAL IMPACT OF ECONOMIC CRISIS IN LATIN AMERICA IN THE 1980s

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Research on the political implications of economic conditions is separated into two relatively distinct bodies of literature. I bridge the theoretical gap between them by examining the effects of economic crisis on electoral outcomes in Latin America from 1982 to 1990. An analysis of 21 competitive elections indicates that crisis conditions undermine support for incumbents and provoke high levels of electoral volatility but without necessarily fostering the growth of political extremism or the exhaustion of elite consensus associated with the breakdown of democracy. The results also suggest that the relationship between economic conditions and electoral instability is mediated by party system structure rather than democratic age. Paradoxically, the findings buttress prior research on electoral outcomes in the comparatively stable and homogeneous Organization for Economic Cooperation and Development (OECD) nations while undercutting theoretical frameworks elaborated with specific reference to the breakdown and consolidation of Third World democracy.

The comparative politics literature is replete with analyses of the relationship between economic conditions and democratic politics. The research is severely bifurcated, however, with scholars specializing in the study of different world regions asking very different sets of theoretical questions. For students of West European and U.S. politics, the electoral implications of economic performance have been a major concern, with debate focused on such issues as the existence of a "political business cycle" or the relative strength of economic and noneconomic voting (e.g., Alt and Chrystal 1983; Beck 1982; Bellucci 1984; Eulau and Lewis-Beck 1985; Franz 1986; Hibbs and Fassbender 1981; Lewis-Beck 1988; Tufte 1978; Visser and Wijnhoven 1990; Whiteley 1986). For analysts of Latin America and other parts of the Third World, on the other hand, electoral issues have been of secondary interest. Scholarly attention has focused instead on the linkage between economic conditions and democratic breakdown, with theoretical controversy revolving around such questions as the relationship between economic underdevelopment and coups d'état (O'Kane 1981, 1983; Londregan and Poole 1990; McGowan and Johnson 1984; Midlarsky and Tanter 1967; Putnam 1967), the political implications of economic dependency (Cardoso and Faletto 1979; Evans 1979), and the nexus between the exhaustion of import substitution industrialization and the rise of bureaucratic authoritarianism (Collier 1979; O'Donnell 1973, 1978; Remmer and Merkx 1982). Whereas hypotheses about politics in the North Atlantic have been formulated predominantly with reference to conditions of relative economic prosperity and stability, the political impact of economic crisis and deprivation have figured prominently in this second body of literature.

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While comprehensible in terms of the varying political realities of the North and South, the existing theoretical division of labor has left comparativists with an impoverished understanding of the impact of economic conditions on democratic governance. Those studying electoral outcomes have largely ignored the impact of economic crises and developed empirical generalizations that apply to but a limited range of economic conditions. Those emphasizing the political significance of economic crises, on the other hand, have devoted little attention to elections. Zimmermann and Saalfeld, whose work represents a major exception to broader research trends, recently underlined this division of labor when they characterized the comparative analysis of the breakdown of European democracy in the 1930s as "among the most underresearched areas in political science" (1988, 305). The same might be said of the comparative study of electoral outcomes in the Third World.

Inasmuch as a key purpose of comparative research is to define "the limits of generalization by specifying the conditions under which hypotheses are valid" (Antal, Dierkes, and Weiler 1987, 14), bridging the theoretical chasm dividing research on the political implications of economic conditions into two distinct bodies of literature is likely to yield significant dividends. This study attempts to take a preliminary step in this direction by addressing a series of questions about the electoral effects of economic crisis: Do the political effects of major economic setbacks parallel the effects of minor ones? Do generalizations about the link between economic conditions and electoral outcomes fit the political experience of non-OECD nations? What factors explain varying political responses to crisis conditions?

Economic Crisis and Politics in Latin America

To link the study of economic crisis with research on democratic elections, the subsequent analysis focuses specifically upon the Latin American experience of the 1980s. During that decade, the region underwent a far-reaching process of political transformation that resulted in the largest and most extended series of competitive elections in its entire history. Simultaneously, however, Latin America was struck by its worst economic crisis since the Great Depression. Following Mexico's mid-1982 declaration of financial insolvency, countries throughout the region began facing acute problems in servicing relatively high levels of accumulated debt with only limited access to fresh external finance. Living standards and investment capacity plummeted in response. As the 1980s drew to a close, the average per capita product of Latin America was 8% lower than at the beginning of the decade, average inflation had surged to the unprecedented level of nearly 1,000%, and the net transfer of resources abroad was continuing at an annual rate of U.S. $25 billion (Economic Commission for Latin America and the Caribbean 1989; hereafter ECLA).

With few exceptions, these economic trends provoked regional specialists to paint a dismal picture of the future of Latin American democracy. Electoral pressures have been seen as undermining the capacity of democratic governments to implement the policies necessary to cope with economic crisis, while lowered living standards, high levels of inflation, and other economic difficulties associated with the net transfer of resources abroad have been linked with the prospect of declining support for democratic rule (e.g., Inter-American Dialogue 1989, ix). Particular fears have been expressed for the future of newly established democracies, which are rather consistently described as "fragile," lacking in legitimacy, and unusually dependent upon their capacity to provide voters with material payoffs (Inter-American Dialogue 1988, 75; Meyer 1989, 37; Seligson and Muller 1987, 322–23; Smith 1989b, 156; U.S. House, Committee on Foreign Affairs 1986, 76; Whitehead 1989, 76–95). The underlying theoretical perspective has been succinctly summarized by Diamond and Linz: "Economic crisis represents one of the most common threats to democratic stability" (1989, 17).

The paradox is that Latin America's worst economic crisis since the Great Depression coincided with the most profound wave of democratization in the history of the continent. Not only did every Latin democracy survive the 1980s, but the number of democracies steadily increased over the course of the decade. By early 1990, when democratic elections paved the way for the transfer of power to civilians in Chile, every established authoritarian regime in the hemisphere had been displaced from power with the exceptions of Cuba and Mexico. These political trends ran counter to prevailing theoretical assumptions and thus raised fundamental questions about the adequacy of our understanding of the relationship between economic crisis and democratic politics.

Among other things, the political experience of Latin America in the 1980s suggests that in the less politically stable regions of the world, "democratic goods" may factor heavily into the calculus of voters, allowing elected leaders, particularly those emerging from a background of dictatorship, to maintain popular support despite acute economic crisis (Hirschman 1987, 28; Kuczynski 1988, 147). Similarly, the unexpected deepening of democratization in Latin America during the 1980s points to the possible limits of generalizations derived from the study of economically stable and prosperous nations. The outbreak of severe economic crisis, as distinct from cyclical economic trends, may dampen popular expectations, providing incumbents with unexpected room for maneuver. If economic setbacks are attributed to international forces beyond local control, incumbents may likewise avoid paying the expected political price for stagnation, inflation, or unemployment.

At present, however, it is only possible to speculate about such issues. Despite the political significance that academics and policy makers have attached to the "lost decade" of Latin American development, comparative analysis of the relationship between democracy and the debt crisis remains limited. Adhering to the research canons of past decades, Latin Americanists have remained more concerned with the etiology of regime change than with the impact of the region's prolonged economic crisis on the process of democratic governance. To the extent that economic conditions have served as a major focus of research, their political implications have been examined principally on a country-by-country basis with minimal overall effort at generalization. Thus, while survey data on countries such as Argentina and Chile have underlined the salience of economic issues in the 1980s, generalizing about the political impact of economic conditions in Latin America remains highly problematic.

Research on Western Europe and North America has done little to fill this void. Despite evidence that the Great Depression provoked major political realignments in democratic countries such as the United States (see, e.g., Brady 1985; Waterman 1990), only a few studies have examined the political repercussions of economic crisis on a comparative basis; and their findings are far from uniform with respect to the political relevance of economic conditions. Particularly significant in this regard is Zimmermann and Saalfeld's recent comparative study of democratic stability in interwar Europe, which concludes that "neither the economic crisis alone nor the economic measures taken suffice to explain the different political outcomes" (1988, 306). The consequences of economic crisis for democratic governance thus remain very much open to debate.

Electoral Outcomes during the "Lost Decade"

Electoral outcomes in Latin America during the 1980s provide a basis for evaluating the implications of economic crisis for democratic governance.[1] Three specific hypotheses are examined:

1. Electoral shifts and volatility vary directly with the magnitude of the economic crisis in the preelection period.
2. The responsiveness of electoral outcomes to economic conditions varies indirectly with the age of democratic institutions.
3. Economic crisis undermines support for established democratic forces and promotes the growth of political extremism.

The first of these hypotheses is drawn from the literature on politics in the OECD nations, which has documented with a relatively high degree of consistency the electoral impact of economic conditions. The other hypotheses are derived from recent literature on Latin American politics, which regards democracies, particularly "new" ones, as extremely vulnerable to economic setbacks.

The data for the analysis, which encompass 21 presidential elections, are drawn from 12 of the 20 nations conventionally included in the Latin American region. Excluded from consideration are nations whose governments were not selected on the basis of competitive elections during the 1980s or nations whose recent electoral history includes only one such election, precluding analysis of electoral change through time. Hence Chile, Brazil, and Guatemala, which in the 1980s held their first competitive presidential elections in a decade or more, have not been included in the study; nor have Cuba, Haiti, Mexico, Panama, and Paraguay, which failed to hold competitive elections. It may be noted that in cases of doubt an attempt has been made to err on the side of inclusiveness. Due to Costa Rica's early entry into the debt crisis, the analysis thus encompasses the Costa Rican election of February 1982, which preceded the formal outbreak of the debt crisis by several months, as well as the Nicaraguan election of 1990, despite the peculiar features of the case associated with the revolutionary origins of its competitive electoral processes. Also initially included in the analysis are the marginal cases of the Dominican Republic, Honduras, and El Salvador, even though competitive elections in these countries have coexisted with widespread allegations of fraud, high levels of military influence over political institutions, limited opposition participation, and outside political intervention.

The 21 presidential elections considered in the study are as follows: Argentina (1989), Bolivia (1985, 1989), Colombia (1986, 1990), Costa Rica (1982, 1986, 1990), the Dominican Republic (1986, 1990), Ecuador (1984, 1988), El Salvador (1989), Honduras (1985 and 1989), Nicaragua (1990), Peru (1985, 1990), Uruguay (1989), and Venezuela (1983, 1988). Nine of these elections took place in countries in which the origins of democratic institutions date back a generation or more (Colombia, Costa Rica, the Dominican Republic, and Venezuela). The remaining 12 elections occurred in the "new" democracies that emerged as part of the post-1979 wave of authoritarian breakdown.

Comparing the economic conditions surrounding these elections poses a number of difficulties, not least because of basic problems of data availability and comparability.[2] To address the three hypotheses, emphasis has accordingly been placed on indicators of inflation, economic growth, and exchange rate depreciation, which are available for all of the cases considered in the study and defined relatively consistently across them. Inflation and economic growth are conventionally accepted indicators of macroeconomic performance that are expected to be reflected in the disposable income of the electorate as well as in assessments of the broader state of the economy. Exchange rate depreciation has long been seen as politically sensitive in Latin America and taps the importance of the international dimension in the evolution of crisis conditions in the region. The inflation and exchange indicators offer the additional advantage of quarterly availability, opening up possibilities for comparing electoral responses over different time periods. While short-term economic setbacks may merely undermine incumbents, recent literature suggests that deeper crises have more generalized effects and lead to electoral disaffection with all established democratic forces.

The specific economic indicators considered in the statistical analysis are as follows: the percentage shift in consumer prices during the quarter prior to the election (or the current quarter if the election was held during its last month), the average annual rate of change in consumer prices over the previous two years, the annual rate of gross domestic product (GDP) growth during the election year (weighted for month of election), the rate of GDP growth during the two previous years, the percentage change in the average exchange rate over the course of the prior quarter (or current quarter if the election was held in its last month), and the total percentage decline in the exchange rate over the previous two years.[3] All data have been drawn from official sources as reported to the International Monetary Fund (1982–90), ECLA (1989), and Inter-American Development Bank (1982–89).

Aggregate electoral change has been measured in terms of three indicators: the percentage of the total vote received by the incumbent party (INCUMVOTE), the percentage shift away from the incumbent party relative to the previous presidential election (INCUMLOSS, which is expressed as a negative figure), and the total percentage shift in votes for all parties between presidential elections. The first two indicators are designed to measure the popularity of incumbents, while the third, which is labeled the *electoral discontinuity index* (EDI) is designed to tap an overall tendency toward electoral volatility as distinct from electoral shifts that merely penalize incumbents.[4] Because the incumbent party's share of the total vote tends to vary with party system structure (as does the potential for electoral change as measured by the EDI), the percentage of the vote received by the two largest parties in the previous election is also included in the analysis. The impact of democratic age has been assessed by counting the number of years democratic institutions had been in continuous existence prior to the election year.

Looking first at Table 1, it is evident that elections held under conditions of economic crisis in Latin America have consistently produced losses for governing parties. Moreover, in the overwhelming majority of cases, elections resulted in the defeat of the governing party or coalition. The key issue, however, is not electoral turnover per se but the relative responsiveness of electoral shifts to economic conditions.

Table 2, which summarizes the bivariate relationships, suggests that strong relationships do exist between economic conditions and electoral outcomes in Latin America. These impressions are sustained by the results of the multivariate analysis, which are based on ordinary least squares (OLS) regression and

Table 1. Electoral Change in Latin America, 1982–90

Country	Year	Incumbent Party	Winning Party	Change in Incumbent Vote
Argentina	1989	Unión Cívica Radical (UCR)	Justicialista	-19.3
Bolivia	1985	Unión Democrática Popular (UDP)	Movimiento Nacionalista Revolucionario Histórico (MNR-H)	-20.9
Bolivia	1989	Movimiento Nacionalista Revolucionario Historíco (MNR-H)	Movimiento de la Izquierda Revolucionaria (MIR)/Acción Democrática Nacionalista (ADN)	-4.8
Colombia	1986	Partido Conservador	Partido Liberal (PL)	-11.0
Colombia[a]	1990	Partido Liberal	Partido Liberal	-10.7
Costa Rica	1982	Partido Unidad Social Cristiana (PUSC)	Partido Liberación Nacional (PLN)	-15.2
Costa Rica	1986	Partido Liberación Nacional (PLN)	Partido Liberación Nacional (PLN)	-6.5
Costa Rica[a]	1990	Partido Liberación Nacional (PLN)	Partido Unidad Social Cristiana (PUSC)	-7.9
Dominican Republic	1986	Partido Revolucionario Dominicano (PRD)	Partido Reformista	-16.7
Dominican Republic	1990	Partido Reformista	Partido Reformista	-5.2
Ecuador	1984	Concentración de Fuerzas Populares (CFP)/ Democracia Popular (DP)	Partido Social Cristiano (PSC)	-14.2
Ecuador	1988	Partido Social Cristiano	Izquierda Democrática	-15.3
El Salvador	1989	Partido Demócrata Cristiano (PDC)	Alianza Republicana Nacional (ARENA)	-7.4
Honduras	1985	Partido Liberal	Partido Liberal	-3.1
Honduras	1989	Partido Liberal	Partido Nacional	-7.2
Nicaragua	1990	Frente Sandinista de Liberación Nacional (FSLN)	Unión Nacional Opositora (UNO)	-25.5
Peru	1985	Acción Popular (AP)	Alianza Popular Revolucionaria Americana (APRA)	-39.1
Peru	1990	Alianza Popular Revolucionaria Americana (APRA)	Cambio 90	-26.5
Uruguay	1989	Colorado	Blanco	-10.9
Venezuela	1983	Partido Social Cristiano (COPEI)	Acción Democrática (AD)	-10.4
Venezuela	1988	Acción Democrática (AD)	Acción Democrática (AD)	-4.3

Sources: Central American Update (Albuquerque), 1990, 7; Corkill and Cubitt 1988, xxi; *El Dia* (Montivideo), 28 November 1989, 1; *Foreign Broadcast Information Service* (FBIS), 14 July 1980, C1; FBIS, 5 April 1984, G1; FBIS, 29 March 1989, 7; FBIS, 26 December 1989, 22; Gamarra 1989; Handelman 1979, 13; *Keesing's Record of World Events,* 24(1978): 28975; 35(1988): 36423; 37(1900): 37371; *La Nación* (Buenos Aires), 15 June 1989, 1; LARR: *Andean Group* 1990e, 6; LARR: *The Caribbean,* 1990d, 4; LAWR 1990a, 11; Leonard and Natkiel 1987; 31–34; Lowenthal 1986–87, D22–23; Roncagliolo 1980, 80–83; *Statistical Abstract for Latin America,* 23 (1984): 729, 731; 24(1985): 171–77; 25(1987): 185, 187, 194, 197; 26(1988): 204, 209–10, 223; 27(1989): 230; *Times of the Americas,* 26 July 1989, 1; Rial 1986, 144.

[a] Provisional results.

Table 2. Bivariate Relationships among Economic Conditions, Party Structure, and Electoral Change

Variables	Incumbent Loss[a]	Incumbent Vote[b]	Electoral Discontinuity Index (EDI)	Inflation[c]	Lagged Inflation[d]	Exchange[e]	Lagged Exchange[f]	Gross Domestic Product (GDP)[g]	Lagged GDP[h]	Major Party Vote[i]
Incumbent loss	—									
Incumbent vote	.71**	—								
Electoral discontinuity index	-.64**	-.67**	—							
Inflation	-.80**	-.48	.58	—						
Lagged inflation	-.75**	-.41	.69**	.89**	—					
Exchange	.76**	.59*	-.50	-.80**	-.75**	—				
Lagged exchange	.72**	.60*	-.44	-.80**	-.73**	.75**	—			
Gross domestic product (GDP)	.33	.36	-.54*	-.36*	-.40	.46	.36	—		
Lagged GDP	.60*	.43	-.61*	-.59*	-.57*	.65**	.52*	.76**	—	
Major party vote	.37	.83**	-.74**	-.33	-.36	.35	.40	.31	.28	—
Years[j]	.43	.59*	-.62*	-.42	-.39	.25	.14	.29	.22	.58*

[a] Percentage change in incumbent party vote between presidential elections (a negative number in all cases).

[b] Incumbent party vote as percentage of total vote.

[c] Percentage change in consumer price index during quarter prior to election (logged to achieve distributional normality).

[d] Average percentage change in consumer prices during previous two years (logged).

[e] Percentage change in exchange rate during quarter prior to election (depreciation indicated by a negative number).

[f] Percentage change in exchange rate during prior two years (logged, with depreciation indicated by a negative number).

[g] Rate of gross domestic product (GDP) growth during previous year (weighted for month of election).

[h] Average rate of growth in GDP during prior two years.

[i] Percentage vote received by two major parties at previous election.

[j] Number of prior years of democratic rule.

*$p \leq .01$.
**$p \leq .001$.

summarized in Tables 3 and 4. Table 3 presents estimations of the models of electoral change that were constructed for the three dependent variables on the basis of the short-term indicators of economic conditions. Table 4 presents estimations based on the two-year indicators of economic change.

Considering the relatively small number of cases, as well as the strong interrelationships among independent variables underlined by Table 2, the results are impressive. As assessed by adjusted R-squared, party structure combined with short-term variations in GDP, inflation, and exchange rates account for 60% of the variation in incumbent vote loss, 74% of the variance in the total incumbent vote, and 67% of the variance in overall electoral discontinuity. The contribution of GDP to these outcomes is minimal. Indeed, two of the three signs for GDP in Table 3 do not even point in the predicted direction. Weak relationships and sign reversals may also be noted with respect to the contribution of exchange rate depreciation to EDI and that of the quarterly rate of inflation to the incumbent vote. The importance of the exchange, inflation, and party structure variables, however, varies from indicator to indicator, with the *t*-ratios indicating that the magnitude of the incumbent vote loss hinges mainly on the rate of inflation, the incumbent vote on a combination of party structure and exchange rate depreciation, and overall electoral discontinuity on a combination of party structure and inflation.

The estimates presented in Table 3 underline the potential electoral costs of high inflation and rapid exchange rate depreciation in the Latin American context. It should be emphasized, however, that the relationships cannot be attributed merely to the unpopularity of devaluation or inflation per se. In the period since 1982, when pressures on the balance of payments have created strong incentives for maintaining price stability and realistic exchange rates, accelerating inflation and associated exchange rate depreciation have been indications of fundamental policy failure and serious overall economic disequilibrium.

The results of the analysis are very similar for the longer-term indicators of economic performance summarized in Table 4. Again, it may be noted that the effects of GDP on electoral change are relatively insignificant and that the relationship between inflation and the incumbent vote, as well as exchange depreciation and EDI, run opposite to the predicted direc-

Table 3. Electoral Outcomes Regressed on Major Party Vote and Short-Term Indicators of Economic Performance

Variables	Constant	Exchange	Gross Domestic Product	Inflation	Major Party Vote	$\hat{R}^2$	F-Ratio	Significance of F	N
Incumbent loss	-8.981	.113	-.079	-3.606	.066	.60	8.50	.0007	21
	(-.96)	(1.32)	(-.21)	(-2.19)	(.63)				
Incumbent vote	-21.512	.193	-.087	.539	.708	.74	15.09	.0000	21
	(-1.96)	(1.93)	(-.20)	(.28)	(5.78)				
Electoral discontinuity index	122.077	.153	-1.942	8.259	-1.120	.67	11.01	.0002	21
	(4.92)	(.68)	(-1.93)	(1.90)	(-4.05)				

Note: Entries are unstandardized regression coefficients with *t*-ratios given in parentheses.

Table 4. Electoral Outcomes Regressed on Major Party Vote and Lagged Indicators of Economic Performance

Variables	Constant	Exchange	Gross Domestic Product	Inflation	Major Party Vote	$\hat{R}^2$	F-Ratio	Significance of F	N
Incumbent loss	-4.488 (-.43)	1.094 (1.45)	.526 (1.12)	-1.731 (-1.66)	.033 (.30)	.57	7.61	.0012	21
Incumbent vote	-20.625 (-1.77)	1.859 (2.23)	.581 (1.11)	1.376 (1.19)	.696 (5.73)	.75	15.79	.0000	21
Electoral discontinuity index	126.330 (6.71)	3.830 (2.83)	-2.342 (-2.77)	7.720 (4.13)	-1.182 (-6.01)	.83	26.20	.0000	21

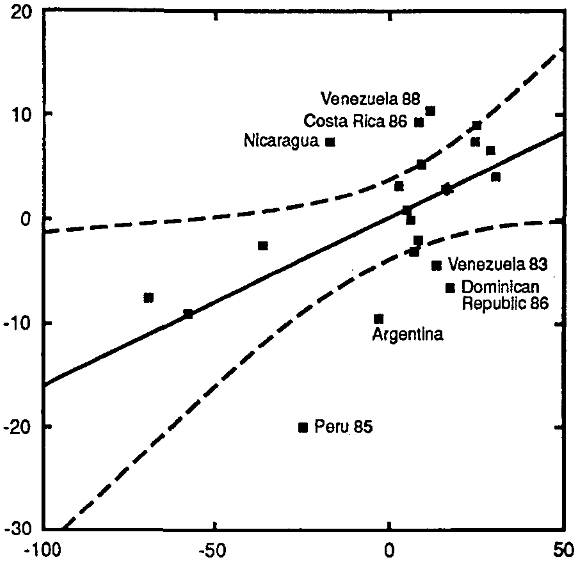
Note: Entries are unstandardized regression coefficients with *t*-ratios given in parentheses.

tions.[5] The key contrast with Table 3 is the estimation for overall electoral change. The latter appears to be far more responsive to economic performance over a two-year period than to fluctuations in the economy immediately preceding elections. These results provide some support for the view that incumbents pay the price for short-term economic setbacks but that deeper crises may be translated into broader political shifts and high overall levels of electoral volatility.

Much of the recent literature on Latin America suggests that new democratic regimes are far more vulnerable to the effects of economic crisis than older and more established democracies. Contrary to these expectations, however, adding years of democratic rule to the regression equations linking electoral change with exchange depreciation does not improve the statistical models. In virtually every instance adjusted R-squared declines, as does statistical significance. The only equation that is enhanced by the addition of years of democratic rule is that linking two-year rates of growth, inflation, and depreciation with incumbent vote; and the gain is marginal. These results suggest that even though average rates of electoral discontinuity have been lower in the region's older democracies than in the newer ones, the differences between the two sets of regimes are not the product of age. Latin American electoral data, consequently, establish no basis for the argument that new democracies are unusually dependent upon what Lipset (1959) calls "effectiveness," as distinct from established legitimacy. The relative immunity of the older democracies to electoral change and volatility instead reflects the impact of party system structure. In this connection it should be emphasized that every "old" democracy in Latin America has a two-party system. Of the eight "new" democracies considered in this study, on the other hand, only Honduras fits squarely within the two-party category. These contrasts suggest that two-party systems in Latin America may be linked not only with greater electoral stability but with more political stability in general.

While the fit of the models estimated in Tables 3 and 4 is not enhanced by the addition of a variable measuring democratic age, there is clearly some room for improving the model specifications through the deletion of variables. A simplified version of the model linking incumbent vote with party structure and

Figure 1. Partial Regression Plot for Exchange



economic conditions provides an illustration. Estimating such a revised model with OLS yields

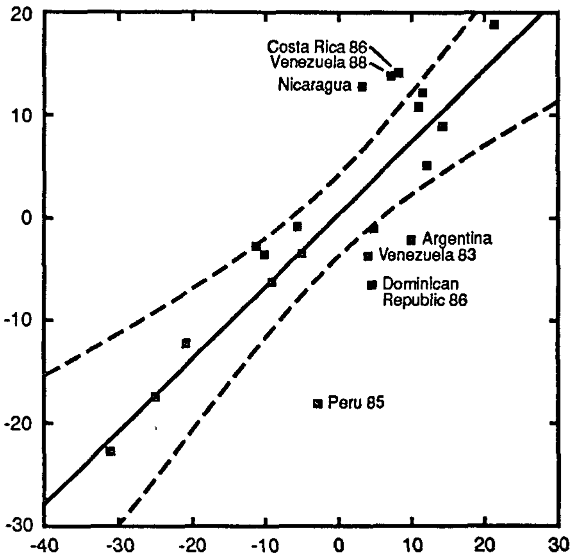
$$\text{INCUMVOTE} = \underset{(-2.083)}{-20.229} + \underset{(6.176)}{.701}\ \text{major} + \underset{(2.888)}{.167}\ \text{exchange} \quad (1)$$

The numbers in parentheses are *t*-ratios, N = 21, and the estimated adjusted R-squared and F-ratio are .77 and 33.7, respectively. Although adjusted R-squared is not much higher than in Table 3, the revised specification represents an advance in the sense that it provides a more parsimonious explanation for variations in the incumbent vote. Similar results can be achieved for INCUMLOSS by deleting the GDP and party structure variables, which raises adjusted R-squared from .600 to .635.

To what extent are the strong linkages among electoral instability, major party vote, and economic conditions a product of particular cases or influential data points? To gain some additional perspective on the robustness of the relationships, Figures 1 and 2 present partial regression plots for the simplified model presented in equation 1, which has the highest adjusted R-squared of any of the estimations that is fully consistent with underlying theoretical expectations.[6] When multivariate relations are involved, the partial regression plot displaying the relationship between two sets of residuals is considered an appropriate graphic alternative to the bivariate scatterplot (Bollen and Jackman 1985). Its utility is well illustrated by Figures 1 and 2, which suggest that the strength of the relationships linking exchange depreciation and major party vote with the incumbent vote is not a product of one or two influential cases; if anything, the opposite is true in the sense that the relationships coexist with the presence of an extreme outlier—Peru.

Peru's position in the lower center of both figures reflects several conditions peculiar to the Peruvian election of 1980, particularly the organizational fragmenta-

Figure 2. Partial Regression Plot for Major Party Vote



tion and financial weakness of the emerging electoral Left. Because of the latter's weak showing in 1980, the indicator of party structure seriously understates the multipolar tendencies of the Peruvian party system. Also relevant to understanding the anomalous position of Peru are the undefined partisan loyalties of newly enfranchised voters and the marked policy failures of the Belaúnde government, which produced a massive electoral repudiation of the Acción Popular party at the polls in 1985. Whereas that party had captured over 45% of the vote in the democratizing election of 1980, its share fell to 6.3% in 1985, placing Peru at the extreme end of the continuum in terms of incumbent vote loss. Eliminating the 1985 election from the analysis consequently improves the fit of all of the models linking electoral outcomes with party structure and economic conditions, including the model presented in equation 1.[7]

Most of the other outliers identified in the partial regression plots are old democracies: the Dominican Republic 1986, Venezuela 1983, Costa Rica 1986, and Venezuela 1988. The first two have a lower vote for the incumbent party than would be predicted by the combination of exchange rate depreciation and party structure, while the latter two have a far higher vote than might have been predicted by these variables. Particularly anomalous are the results of the 1988 Venezuelan elections, which, like those of the Peruvian election of 1985, are reflected as high residuals in all of the models linking electoral outcomes with economic conditions.

Data problems are partially responsible, particularly in the case of Venezuela. Whereas International Monetary Fund data indicate that the Venezuelan exchange rate was stable prior to both the 1983 and 1988 elections, that stability was the artificial product of a controlled foreign exchange market and quite misleading with respect to the actual state of the economy. In 1983, Venezuela confronted a critical external balance of payments crisis as a result of heavy external debt obligations, capital flight, and declining export earnings. Indeed, 1983 might be described as the most disastrous year in recent Venezuelan economic history. By the time of the elections, devaluation was regarded as all but inevitable (see, e.g., Economist Intelligence Unit 1983, 7). In contrast, the 1988 election, in which the incumbents received a greater-than-predicted percentage of the vote, took place at the end of a year of economic recovery and expansionary government policies. The electoral losses of incumbents in the case of Venezuela are consequently consistent with the general findings linking economic conditions with aggregate electoral outcomes.

The limitations of the exchange rate indicator are also evident in the cases of Argentina and the Dominican Republic, in which the incumbent vote falls significantly below the predicted level. In the Dominican case, high levels of official capital in-flow contributed to currency appreciation in the months before the 1986 election, leading the exchange rate indicator to provide a somewhat optimistic indication of the overall condition of the economy (Economist Intelligence Unit 1986, 19). The same is true of the Argentine case, where the economy was teetering on the brink of acute crisis by the eve of the 1989 election.

The outliers that emerge in Figures 1 and 2, however, cannot be attributed solely to the limitations of available economic indicators. Even under crisis conditions, the state of the economy is not necessarily the only political issue of relevance to electorates. The important role played by personality in presidential systems is reflected in the outcome of the Venezuelan election of 1988, in which the incumbent Acción Democrática's standard-bearer, former president Carlos Andrés Pérez, received a far higher percentage of the vote than might have been predicted on the basis of the economic performance of the incumbents. Personality issues also played a role in the 1986 Dominican elections, in which a bitter split in the incumbent Partido Revolucionario Dominicano over the presidential nomination paved the way for the emergence of a significant third-party vote. The drop in the incumbent vote was accordingly larger than could have been predicted on the basis of economic performance and preexisting party structure. In addition, economic issues have been subordinated to questions of war and peace, as reflected in the residuals for the 1986 Costa Rican election. Available survey data indicate that the debt crisis was not the dominant issue in that election (Nelson 1989, 157). By the mid-1980s the country had weathered the worst of the economic crisis and achieved a political consensus on appropriate responses. The issue that divided the two major parties was less the economy than the Central American political crisis, which permitted the incumbent Partido Liberación Nacional to capitalize on the electoral appeal of their regional peace proposals (see *Latin America Regional Report: Mexico and Central America,* 1986, 7; hereafter *LARR*).

The case of Nicaragua, which also appears as a significant outlier in Figures 1 and 2, poses another set of issues having to do with the inclusion of marginal cases of democracy in the analysis. However honest the balloting processes of 1984 and 1990, the Frente Sandinista de Liberación Nacional's (FSLN) leadership of the 1979 revolution created major power asymmetries between government and opposition forces, which are reflected in the residuals plotted in Figures 1 and 2. In 1984 the FSLN won 67% of the vote in an election that was competitive only in the technical sense of that term. Despite massive economic setbacks and the related alienation of a significant proportion of its base of electoral support, the party retained the loyalty of approximately 40% of the electorate when it was defeated in 1990. The position of the Nicaraguan case in Figures 1 and 2 thus reflects the peculiarities of a postrevolutionary political situation. The case thereby brings to light possible distortions in the analysis resulting from the inclusion of relatively marginal instances of democratic rule from the Central American and Caribbean region.

Do the relationships between electoral outcomes and economic conditions remain significant if such cases are eliminated from the analysis? Although the number of cases is significantly diminished, the answer is *yes*. After excluding the six elections taking place in Honduras, El Salvador, Nicaragua, and the Dominican Republic, the revised estimates for incumbent vote regressed on major party vote and exchange, with the *t*-ratios in parentheses, are

$$\underset{}{\text{INCOMVOTE}} = \underset{(-2.170)}{-22.873} + \underset{(5.879)}{.729}\ \text{MAJOR} + \underset{(2.778)}{.176}\ \text{EXCHANGE}$$

The equation has an adjusted R-squared of .803 and an F-ratio of 29.531, which is statistically significant at the .0000 level. The results are even more positive for the models linking overall electoral discontinuity with economic conditions. For conditions over a two-year period, adjusted R-squared falls slightly, from .834 to .805; but the theoretical fit is better, with the two-year change in the exchange rate negatively linked with EDI. The same is true with respect to short-run economic conditions; moreover, the adjusted R-squared of the model linking economic conditions with EDI increases from .667 to .775. In terms of the indicator of incumbent losses, the elimination of the six Caribbean Basin cases slightly weakens the relationships, although they remain statistically significant.

These results imply that the relationship between economic conditions and electoral outcomes is not a function of the inclusion of the Central American region or marginal democratic cases. As in the United States and Western Europe, the electoral fate of incumbents in Latin America has been significantly affected by economic performance. Contrary to most of the literature on Latin America, however, the strength of the electoral response to economic conditions has not been mediated by the age of democratic institutions but by party system structure. Aggregate electoral data thus provide no evidence that "new" democracies are peculiarly susceptible to fluctuations in economic conditions.

Economic Crisis and Political Extremism

If economic crisis merely produces electoral turnover, diminishing opportunities for *continuismo*, or the perpetuation of rule by incumbents, economic adversity might be seen as strengthening the viability of democratic institutions, particularly in regions where respect for competitive norms has not been well established. The recent literature on Latin America implies, however, that economic crisis provokes not merely electoral turnover but democratic instability. From this perspective, the critical question is not the strength of the tendency toward economic voting under crisis conditions but the extent to which those conditions reduce support for established democratic forces, strengthen the political extremes, and provoke the breakdown of democratic consensus, as in Austria and Germany during the interwar period.

Available survey data establish no basis for linking the Latin American economic crisis of the 1980s with diminished popular support for democratic institutions. The case of Argentina is particularly telling in this regard—and not merely

Table 5. The Evolution of Public Opinion in Argentina (%)

Statement	May 1983	May 1984	April 1985	April 1986	March 1988	June 1988
Personal economic situation is worse than last year	—	26	42	34	—	62
Government is solving or with time will solve economic problems	—	79	65	63	24	—
The best political system is based on periodic elections	79	83	—	84	—	79

Source: Catterberg 1989, 44, 48, 64.

because data have been collected that make it possible to explore fluctuations in citizen support for democratic institutions through time. To use Eckstein's (1975) terminology, Argentina is a "critical case" in the sense that it represents one of the most likely instances—if not *the* most likely instance—of an economic crisis eroding support for democratic institutions. Argentina is a country historically characterized by the exceptional fragility of representative institutions; its current regime is a "new" democracy; and the nation falls near the extreme end of the spectrum in terms of recent economic policy failures. Over the course of the 1982–89 period, per capita income declined at an average annual rate of 2.2%—among the worst records in the hemisphere—without any perceptible compensation in terms of economic stability. By 1989 the annual rate of inflation was approaching 4,000%, the highest figure in the region (ECLA 1989, 19–20; see also Smith 1990; Pablo 1990). Not surprisingly, during the 1980s the Argentine electorate consistently identified the economic situation as the most serious problem facing the nation. Equally understandably, optimism about the capacity of the government to improve the economic situation diminished steadily through time. Nevertheless, the level of popular support for democratic institutions remained relatively constant. As indicated in Table 5, the percentage of the electorate agreeing that the best political system is one based on periodic elections was 79% in May 1983, before democratic institutions had been fully restored, and exactly the same percentage in June 1988, when 62% of survey respondents were describing their personal economic situation as worse than in the previous year.

The Argentine example is not an isolated one. Survey data for Peru, a country whose history of democratic fragility and recent policy failures rival those of Argentina, show a similar pattern of continuing support for democracy rule in the face of prolonged and steadily deepening economic crisis. According to data assembled by McClintock (1989, 359), support among Lima residents for a democratic regime grew from 66% in February 1982 to 81% in April 1988, while the percentage of respondents expressing a preference for socialism, military rule, or other alternatives declined. The pattern was not appreciably different from that documented in the case of Costa Rica, which is arguably the best-established Latin democracy (see Seligson and Muller 1987; see also Seligson and Gómez B. 1989, 177–78).

These findings have significant implications. If a decade of dismal economic performance has failed to erode support for democracy in Peru and Argentina—countries that stand out in terms of past democratic instability as well as recent economic policy failures, there seems little reason to suppose that economic crisis has significantly undermined popular support for democracy elsewhere in the region. Survey data thus conform to the line of analysis developed by Linz and Stepan (1989, 42–48), who argue that democratic legitimacy and perceptions of socioeconomic efficacy are not tightly coupled but are instead mediated by evaluations of the specific alternative political formulas of the recent past and plausible future.[8] The ability of democratic rulers to claim ruling authority on the basis of procedural origins rather than governmental performance per se and the possibility of democratic alternation of government are likewise seen as factors insulating democratic regimes from disaffection based on unsatisfactory policy performance.

The fate of democracy, however, is arguably less a matter of popular support, or "legitimacy," than of questions of polarization and consensus. In 1973, for example, Chilean and Venezuelan citizens expressed the same level of support for democratic institutions (Huneeus 1986, 66; Levine 1987, 269); but whereas Chilean democracy succumbed to a military coup in September of that year, Venezuelan democracy continued to thrive into the 1990s. The critical difference between the two countries was not popular support for democracy as a system of governance but the level of political polarization, which reached extreme levels in Chile.

To address the important issue of polarization, Table 6 presents data on shifting patterns of support for minor parties. While such data only capture electoral expressions of political extremism, the growth of parties representing the ends of the political spectrum is a clear indication of political polarization and one that may be expected to reflect, as well as influence, other dimensions of the political situation. Perhaps the most striking feature of the table is the large number of negative entries in the second column. In more than half of the elections held after outbreak of the debt crisis, minor parties lost ground; and in all but one of these cases (El Salvador), the drop in the minor party vote can be attributed to the strengthening of centrist forces. Minor parties appear to have achieved major gains at the expense of better-established parties in only six countries: Argentina (1989), Bolivia (1989), Colombia (1990), Dominican Republic (1986), Ecuador (1984), and Peru (1985); and in only one of these six does the gain in the minor party vote establish a basis for linking the debt crisis with the growth of political extremism.

In Argentina, the increase in the minor party vote share between 1983 and 1989 mainly reflected the development of a democratically oriented and business-linked party, the Unión del Centro Democrático. As indicated, in the Dominican case clashes over the presidential nomination paved the way for the electoral upsurge of a third-party force, the Partido Liberación Dominicana: a populist party closely identified with its long-standing leader, Juan Bosch. In Colombia, the removal of barriers to the electoral participation of the Left, rather than major shifts in the relative strength of political forces, was the principal factor behind the growth of the minor party vote.

Reflecting the unstable and highly fragmented character of their party systems, the causes of the upward shift in minor party vote were more complicated in Bolivia and Ecuador; but in neither case can the shift be attributed to political extremism. Two major changes occurred in Bolivia between 1985 and 1989. One was a doubling of electoral support for the Movimiento de la Izquierda Revolucionaria, which can probably best be described as a social democratic or Center–Left party. The other was the emergence of a new personalist party, the Conciencia de Patria, which brought together a disparate array of elements behind the candidacy of a popular talk show host, Carlos Palenque. Taken together, these two changes might be seen as reflecting popular disillusionment with established political forces. On the other hand, in 1989 the three largest parties together received 72.6% of the total vote as compared to 73.4% in 1985: hardly a symptom of a dramatic shift away from established democratic forces. The Ecuadoran case is very similar. The upsurge in the minor party vote in the 1984 election reflected a strengthening of the democratic Left (the Izquierda Democrática) and the emergence of new centrist forces rather than the growth of political extremism. Thus, in five of the six cases in which minor parties achieved major gains, the dominant motif was continuing, or even increased, support for centrist rather than extremist political options.

The case that stands out as an exception to these trends is that of Peru, in which the expansion of the minor party vote reflects the rise of a new electoral force, which, while not necessarily extremist, was unambiguously left-wing rather than

Table 6. Shifts in Electoral Support for Minor Parties and Party Coalitions (%)

Country and Year	Total Minor Party Vote[a]	Change[b] (%)	Leading Minor Party	Ideological Position
Argentina 1989	20.3	+12.1	Unión del Centro Democrático	Center-Right
Bolivia 1985	36.8	−5.9	Movimiento de la Izquierda Revolucionaria	Center-Left
Bolivia 1989	27.3	+12.3	Conciencia de la Patria	Populist
Colombia 1986	5.8	−6.4	Unión Patriótica	Left
Colombia 1990	15.2	+9.4	Alianza Democrática/Movimiento 19 de Abril	Right Left
Costa Rica 1982	7.5	−1.4	Partido Movimiento Nacional	Right
Costa Rica 1986	2.0	−5.5	Alianza Popular	Left
Costa Rica 1990	1.4	−0.6	Pueblo Unido Coalición	Left
Dominican Republic 1986	26.0	+15.4	Partido de la Liberación Dominicana	Populist
Dominican Republic 1990	7.7	−0.1	Partido Revolucionario Independiente	Populist
Ecuador 1984	44.3	+33.7	Concentración de Fuerzas Populares (CFP)/Democracia Popular (DP)	Center-Right
Ecuador 1988	42.8	−2.3	Acción Popular Revolucionaria Ecuadoriana	Populist
El Salvador 1989	10.2	−1.4	Partido de Conciliación Nacional	Center-Right
Honduras 1985	3.5	−0.6	Partido Demócrata Cristiano (PDC)	Center
Honduras 1989	4.5	+1.0	Partido Demócrata Cristiano (PDC)	Center
Nicaragua 1990	4.5	−28.5	Movimiento de Unidad Revolucionaria	Center-Left
Peru 1985	33.0	+20.2	Izquierda Unida (IU)	Left
Peru 1990	28.6	−4.4	Izquierda Unida (IU)	Left
Uruguay 1989	9.0	+6.5	Partido Gobierno del Pueblo	Center-Left
Venezuela 1983	8.0	−3.5	Movimiento al Socialismo (MAS)	Left
Venezuela 1988	6.6	−1.4	Movimiento al Socialismo (MAS)	Left

Source: Same as for Table 1.

[a] Total vote received by parties or coalitions supported by less than 15% of the electorate.

[b] Total percentage change in vote for parties or coalitions that received less than 15% of the vote for the prior presidential election.

center-leftist in its orientation. Between 1980 and 1985, the Izquierda Unida expanded its base of support from just under 3% of the electorate to over 20% in 1985. Concomitantly, the vote received by the two major parties in 1980 fell from 72.8% to 52.0% of the total vote. These trends were reflected in developments outside of the electoral arena, most notably in the emergence of a serious guerrilla challenge to democratic institutions. In terms of the cases considered here, the closest parallel is El Salvador, where the loss of support for centrist forces in the election of 1988 went hand in hand with guerrilla violence. In the Salvador case, however, the tendency toward political polarization and violence was well established before the onset of major economic difficulties.

Is the Peruvian case anomalous, or does the growth of political extremism there prefigure developments in other parts of the region? Although the linkage between economic crisis and electoral discontinuity so evident in the Peruvian case appears to be characteristic of Latin America as a whole, the tendency toward political extremism does not. Factors specific to the Peruvian experience, particularly the legacy of economic policy failure and left-wing mobilization left by the military regime of the 1968–75 period, are critical to understanding the upsurge in left-wing voting that occurred in Peru during the early 1980s (see Booth and Sorj 1983; FitzGerald 1979; McClintock and Lowenthal 1983; Stepan 1978; Stephens 1983; Thorp 1979; Wise 1989). The significance of the growth of extremist forces in the 1985 Peruvian election is further undercut by the outcome of the subsequent presidential election, in which Peruvians abandoned political forces on both the Left and the Right and expressed a clear preference for moderate solutions to the ongoing crisis.

Patterns of government formation in Latin America during the 1980s provide additional evidence of a trend toward consensus formation rather than political polarization. Far from evincing growing symptoms of political extremism and elite dissension, the 1980s were characterized by unprecedented efforts at achieving national political consensus. Among the most notable of these efforts was the Bolivian Pact for Democracy of 1985, under which the newly elected Movimiento Nacional Revolucionario and the leading opposition party formally agreed on a series of measures to cope with the debt crisis. The pact marked the first occasion in Bolivian history when government and opposition forces agreed on a common set of policy objectives (Gamarra 1989; see also Conaghan, Malloy, and Abugattas 1990). Following the election of 1989, the process of national consensus formation was carried forward by an agreement between the formerly antagonistic Acción Democrática Nacionalista and Movimiento de la Izquierda Revolucionaria on the creation of a government of national unity (*LARR: Andean Group* 1989b, 1–2).

The Bolivian experience is not unparalleled elsewhere in the region. As the debt crisis persisted through time, progressively eliminating perceived alternatives to the "Washington agenda" of externally monitored programs of economic austerity, liberalization, and adjustment (see Williamson 1990), elites in country after country turned to pacts as a means of establishing a base of support in the face of politically unpalatable policies. In Chile, a coalition of 16 opposition parties reached agreement on a common political strategy and socioeconomic program in 1988, paving the way for the defeat of the Pinochet dictatorship in the so-called "Campaign of the No" and the subsequent election of a broad coalition government (see Garretón 1988; *LARR: Southern Cone* 1990a, 1–2; Stallings 1989). In Argentina, crisis conditions provoked not only efforts at *concertación social,* or pact making, among labor organizations, business groups, and state elites but also an incongruous alliance between the governing Peronists and their erstwhile nemesis, the head of the Union del Centro Democrático (see Riz, Cavarozzi, and Feldman 1987; Fernández 1987; *LARR: Southern Cone* 1990b, 2; *Latin American Weekly Report* 1986c, 8 [hereafter *LAWR*]; *LAWR* 1990b, 2; Smith 1989a, 266–97). In Colombia the traditional power-sharing agreement between the two major parties was continued through the 1980s and then broadened following the election of President César Gaviria with the appointment of a cabinet including seven Liberals, four Conservatives, a military officer, and a member of the former guerrilla organization M-19 (*LAWR* 1990d, 8). The 1980s also witnessed path-breaking—albeit not necessarily successful—efforts at consensus formation in Uruguay (Anglade and Fortín 1990, 291–92; *LARR: Southern Cone* 1990b, 3; 1990c, 7; 1990d, 6; *LAWR* 1986a, 3), Venezuela (*LARR: Andean Group* 1989a, 2), Honduras (*LAWR* 1989, 9), the Dominican Republic (*LAWR* 1986b, 10), Nicaragua (*LAWR* 1990e, 1), and even, somewhat improbably, Peru, where in mid-1990 the newly elected president, Alberto Fujimori, proceeded to name a cabinet that included representatives of political parties on both the political Left and Right (see "Concertación total" 1990; *LAWR* 1990c, 1). Whatever the long-term success of these efforts, the history of Europe in the interwar period underlines their potential importance. Only a minority of European democracies weathered the crisis of the 1930s. According to recent research, those that did were distinguished less by their initial economic situation or policy responses to the Great Depression than by "processes of national consensus formation among political and economic elites" (Zimmermann and Saalfeld 1988, 308; on the importance of consensual agreements among elites, see also Higley and Burton 1989).

The significance of elite pacts may be even more profound in the contemporary Latin American context. Not only have they created the political space necessary to adopt drastic policy measures, pacts have also inhibited popular participation in policy formation processes, thereby offering guarantees of economic policy continuity and limited social redistribution to propertied elites who have historically mounted the major challenges to democracy in the region. Latin Americanists have long recognized the importance of such guarantees, as well as the role of pacts in transitions from authoritarianism (Karl 1986; Levine 1978; O'Donnell 1986, 11–15; O'Donnell and Schmitter 1986, 37–47); but the relationships that developed in the 1980s among economic crisis, elite consensus, and democratic rule have been insufficiently analyzed. To the extent that elite pacts may be seen as not merely imposing limits on democratic rule but also facilitating its consolidation (as suggested by most—albeit not all—recent literature),[9] the debt crisis may paradoxically prove to have created an unusually favorable set of opportunities and incentives for democratic development in Latin America.

Conclusions

During the 1980s economic crisis provoked electoral instability and turnover in Latin America. The magnitude of electoral change was directly related to the depth of the crisis experienced in the preelectoral period, with variations in exchange rates, GDP, and inflation highly correlated with various indicators of electoral outcomes. The relationship between economic conditions and aggregate electoral results was mediated principally by party system structure, which insulated two-party democracies from the volatility experienced by their more politically fragmented counterparts. Contrary to the hypotheses of most regional observers, however, the sensitivity of electoral outcomes to economic conditions was not significantly affected by the relative age of Latin democracies. In terms of electoral responses, the so-called new democracies do not stand out as a distinctive group that can be characterized as unusually vulnerable to economic reversals. Available data also undermine arguments linking the debt crisis with the growth of political extremism, political polarization, and democratic instability. The prolongation of the crisis may well have more corrosive effects; nevertheless, political trends in the 1980s ran counter to most predictions. As indicated by electoral outcomes and related patterns of government formation, democratic politics in Latin America were characterized less by the expansion of extremist forces than by the consolidation of support for centrist options and the unprecedented search for elite consensus.

These findings have significant implications for the study of liberal democracy. The responsiveness of electoral outcomes to economic reversals in Latin America reinforces prior research, emphasizing the broader theoretical relevance of findings grounded in the political experience of the comparatively homogeneous and stable OECD nations. At the same time, the findings undercut a second body of literature on the breakdown and consolidation of democracy. Whereas research on Latin America and other parts of the Third World has repeatedly linked economic reversals with democratic collapse, the Latin American experience of the 1980s suggests that economic crisis should be described less as a threat to democracy than as a challenge posing opportunities as well as risks. Economic decline may menace democratic stability by provoking political polarization, but it also may create new incentives and opportunities for building elite consensus around democratic institutions.

In terms of democratic theory, a further implication of the study is that established notions regarding the contributions of legitimacy and effectiveness to democratic stability are unhelpful and potentially misleading. The principal difficulty is that legitimacy has been seen as a time-dependent outcome, such that new democratic regimes are all-but-defined as fragile, lacking in legitimacy, and dependent upon policy effectiveness.[10] As the introduction to a recent comparative volume on Third World democracy concedes, "So intimately is legitimacy tied to democratic stability that it is difficult to know where definition ends and theorizing begins" (Diamond, Linz, and Lipset 1990, 9). Legitimacy reflects stability (or persistence through time), and stability reflects legitimacy. One result is a tendency toward tautology; another is an inability to grapple with facts that do not fully conform to assumptions regarding the importance of democratic age or persistence. The ability of new Latin American democracies to weather prolonged economic crisis is one such fact—a fact that does not easily square with the supposed dependence of new regimes on effectiveness or performance. The concept of legitimacy offers insights into this situation only insofar as it is uncoupled from the concept of stability or regime persistence. Support for competitive institutions, particularly at the elite level, is a key issue; but as Linz and Stepan (1989) have emphasized, such support needs to be understood in terms of recent political experience and perceived political alternatives to democracy. Those alternatives, which condition prospects for future democratic breakdowns, are likely to seem least attractive where recent authoritarian experience proved most repressive and costly.

The problems of emphasizing democratic age over other system properties are specifically underlined by Latin American electoral results. During the 1980s neither the capacity of incumbents to maintain a base of political support in Latin America nor the sensitivity of electoral outcomes to economic conditions or policy performance varied with the age or presumed legitimacy of democratic institutions. The assumption that political leaders in new democracies will be peculiarly predisposed to succumb to the temptations of economic populism thus seems inappropriate, as does the assumption that "new" can be readily equated with "fragile." Democracy may be more difficult to sustain in some nations than in others, lending plausibility to potentially circular arguments linking accrued legitimacy with stability; but newly established democracies do not appear to function according to a separate set of rules. It is precisely for this reason that the literature grounded in the political experience of the United States and Western Europe offers more important insights into the politics of Latin America during the 1980s than theoretical frameworks elaborated with specific reference to the relationship between economic crisis and democracy in the region.

Notes

I am grateful for the assistance of Hank Jenkins-Smith and Catherine Hansen in the preparation of this study.

1. Following the conventions established in the study of Latin American politics over the course of the past two decades, democratic governance is defined here strictly in institutional terms, leaving open to empirical investigation questions regarding the consequences of competitive institutions for popular participation in policy formation, socioeconomic equity, and other political outcomes. For further definitional specification and discussion of the advantages of the institutional concept of democracy, see Karl 1990, 1-2. See also Diamond and Linz 1989, 1-58; Huntington 1989, 11-28; O'Donnell and Schmitter 1986, 7-8.

2. To provide some idea of the dimensions of this problem, official real-wage data are available for only 9 of the 21 cases; and the definition of real wages varies considerably across those 9. Whereas some countries report national data, others calculate changes in real wages on the basis of trends in a single metropolitan area. Similarly, the index variably refers to wages in the manufacturing sector, wages of private-sector manual workers, remunerations of workers covered by the social security system, and wages in all nonagricultural sectors.

3. The exceptions, which reflect problems of data availability, are twofold: (1) short-term inflation rates for the Nicaraguan and Peruvian elections of 1990 represent shifts in consumer prices two quarters prior to these elections, and (2) economic growth rates for all 1990 elections have been calculated on the basis of 1989 data.

4. The index, which is identical to that I utilized in Remmer 1985, has been constructed by adding the percentage shift in the vote for small political parties (those receiving less than 5% of the total vote) to the sum of the percentage shifts in vote shares for each party receiving more than 5% of the vote at either the baseline or subsequent election as illustrated by the following hypothetical example:

Party	% Vote Election 1	% Vote Election 2	% Change
Conservative	40	50	10
Liberal	30	30	0
Socialist	30	10	20
Minor parties	0	10	10

EDI = 40

5. It may be noted that of these, the only significant inconsistency is that relating to EDI. The relationships between economic conditions and EDI, however, remain relatively robust with the elimination of the exchange variable.

6. To obtain the partial plot for exchange, two regressions are estimated:

$$Y = b_0 + b_1X_2 + e \tag{2}$$

and

$$X_1 = c_0 + c_1X_2 + e, \tag{3}$$

where Y is INCUMVOTE, X_1 is the quarterly change in the exchange rate, and X_2 is the major party vote. The partial plot for exchange is formed by arraying the residuals from equation 2 against those for equation 3. Similarly, the partial plot for major party vote relates the residuals from the regression of INCUMVOTE on exchange to the residuals from the regression of major party vote on exchange.

7. With N = 20, the adjusted R^2 for equation 1 increases from .77 to .81 and the F-ratio from 33.7 to 41.4. For the three sets of estimates presented in Table 3, the adjusted R^2 for INCUMLOSS increases from .600 to .730, for incumbent vote from .738 to .799, and for EDI from .667 to .701. Changes are of a similar magnitude for the estimates presented in Table 4.

8. For a discussion emphasizing the importance of perceived alternatives, see also Przeworski 1986.

9. An interesting exception, Smith (1989a, 267–97) argues that constraints on participation in decision-making processes may jeopardize the process of democratic consolidation.

10. For example, according to Diamond, Linz, and Lipset, democratic stability (defined as "persistence and durability . . . over time") "requires a widespread belief among elites and masses in the legitimacy of the democratic system." A *stable* regime is "one that is deeply institutionalized and consolidated, making it likely to enjoy a high level of popular legitimacy," whereas "*unstable* regimes are, by definition, highly vulnerable to breakdown or overthrow in periods of acute uncertainty and stress. New regimes, including those that have recently restored democratic government, tend to fall in this category" (1990, 9; emphasis original). For another recent and very similar formulation of these issues, see Seligson and Muller 1987. See also Whitehead (1989). Although he eschews the term *legitimacy*, he argues that the process of democratic consolidation involves "a deepening of the commitment of most actors to the mutually negotiated democratic framework." In Whitehead's view, "it is unlikely that such a process can ever be fully accomplished in less than a generation" (p. 79).

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THE DYNAMICS OF POLITICAL CONTROL OF THE BUREAUCRACY

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A new paradigm of political-bureaucratic relations emerged through the 1980s holding that U.S. democratic institutions continuously shape nonelective public bureaucracies. Several empirical studies support the paradigm with evidence suggestive of political manipulation but none reveals the scope or specific mechanisms of political control. We explore the dynamics of political control of the bureaucracy explicitly to determine the scope and mechanisms. We examine output time series from seven different public bureaucracies for responsiveness to political tools applied in the late Carter and early Reagan administrations. We find responsiveness in all seven cases. The evidence also shows that political appointments—a shared power of the president and Congress—is the most important instrument of political control; changing budgets, legislation, congressional signals, and administrative reorganizations are less important. These findings confirm intuitive assertions by institutional scholars and suggest a method of "policy monitoring" that could enhance future democratic control of the bureaucracy.

Through the 1980s theories of political control of the bureaucracy underwent extraordinary change. The dominant paradigm shifted from one emphasizing the extreme difficulty U.S. elected institutions have in directing a massive and lumbering federal bureaucracy to one emphasizing that elected leaders can and do shape bureaucratic behavior in systematic ways. In part this change resulted from the election of a Republican president and Senate in 1980. History demonstrated that political control was possible since the Reagan administration seemingly changed many programs. Equally important, however, was that social science developed new tools for evaluating the extent of political control of the bureaucracy. Scholars increasingly viewed the control problem as an economic one involving manipulation of microlevel incentives and constraints. They also used dynamic methods to examine the substance of political-bureaucratic relations.

We will present results on the dynamics of political control of the bureaucracy from our study of seven different federal agencies. The findings confirm past research on assorted agencies showing that bureaucracies do respond to external politics. Since our sample of agencies is large, the results demonstrate the scope of political control. However, our main contribution is in showing the causal mechanisms of political control across multiple agencies. We empirically determine *how* political control occurs, as well as the relative importance of various political stimuli (i.e., appointments, budgets, reorganizations, oversight, and legislation). The findings increase scientific knowledge

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of U.S. democracy. They may also be useful in assuring official accountability and in developing monitoring and feedback systems for future democratic control.

Theories of Political Control

Before the 1980s most research on political-bureaucratic relations was qualitative, seldom considering actual covariations between political stimuli and measurable bureaucratic outputs. For example, the politics–administration dichotomy, which was the dominant paradigm in the public administration literature until well into the 1940s, dispensed a priori with any possibility of political influence on the bureaucratic state. During the 1950s and 1960s the iron triangle or capture theory replaced the politics–administration dichotomy. Capture theory acknowledged that politics was important but argued that the policy loop excluded major political actors, most notably the president (Cater 1964; Freeman 1965; McConnell 1966). As a result of these precepts, many presidential studies took the position that presidents lacked the resources to monitor and control the federal bureaucracy effectively (e.g., Cronin 1980; Fenno 1959; Koenig 1975; Noll 1971; Rossiter 1956). The president was perceived as a disinterested bystander with limited influence.

Similarly, the literature on Congress described difficulties with legislative control mechanisms. Scher (1960) wrote that members of Congress are concerned more with satisfying electorates than with overseeing the bureaucracy. He observed a lack of policy-directing oversight by congressional committees. Others raised questions about the quality of legislative oversight, noting that it is uncoordinated, fragmented, and ad hoc (Bibby and Davidson 1972; Dodd and Schott 1979; Fiorina 1981; Ogul 1976; Sundquist 1981). Similarly, the literature on the congressional appropriations process cast doubt on the budget as an effective tool of control. Wildavsky (1964) and Fenno (1966) described appropriations as based on simplifying decision-making rules rather than rational decisions for controlling public policy. Budgeting was decentralized and incremental, resulting in automatic increases that further insulated the bureaucracy from political control.

Qualitative assessments before the 1980s saw neither the president nor Congress as an effective institution for central control of the bureaucracy. Two developments led to a change in this perception: an economic theory of political–bureaucratic relations and a growing body of empirical support for that theory. *Agency theory* originated in the management literature, where scholars developed principal–agent models to assess relations between private business leaders and their employees. However, political scientists studying politics and bureaucracy saw these models as directly applicable to the problem of political control (see Moe 1984 for a summary).

The application of agency theory to politicians and bureaucrats has several key assumptions. First, the relationship between elected leaders and nonelective bureaucrats is hierarchical. Bureaucratic agents are bound by contract to serve democratic principals. Their primary responsibility is faithful implementation of the law. Second, through time there develop disjunctures between the interests of politicians and bureaucrats. Political coalitions change from those existing when democratic principals initially adopt policies. Similarly, bureaucracies develop separate interests through institutionalization and external politics. Third, politicians will often try to change policy implementation. However, since bureaucrats have separate interests, they will often attempt to shirk external demands. The key question posed by theory is how politicians vested with contemporaneous legitimacy can overcome this shirking tendency.

The answer supplied by agency theory is that political control is possible because elected institutions create bureaucracies. They design bureaucracies with incentive structures to facilitate control. Political principals also monitor bureaucratic activities to offset information imbalances. When bureaucratic activities stray from the desired result, policy makers apply sanctions or rewards to bring them back in line. Thus, the theory is dynamic, positing well-informed central decision makers who systematically mold the preferences of bureaucratic agents.

Several studies operating under the assumptions of agency theory found evidence suggestive of political manipulation. For example, Moe (1982) analyzed annual outputs from the Federal Trade Commission (FTC), the National Labor Relations Board (NLRB), and the Securities and Exchange Commission (SEC) and found they varied with changing presidential administrations. Weingast and Moran (1983) used data on FTC decisions and found they varied with the ideology of congressional oversight committees. Moe (1985) employed quarterly data on NLRB decisions and found they varied with features of all three major U.S. institutions (i.e., the president, Congress, and courts). Others examined political control of bureaucracies in an intergovernmental context and found covariation in state programs with both national and subnational politics (Chubb 1985; Scholz, Twombly, and Headrick 1988; Scholz and Wei 1986; Wood 1991a; 1991b).

Still, the evidence on political manipulation remains indefinite, with many unanswered questions. What is the *scope* of political control of the bureaucracy? Is it limited to the few agencies examined by past research, or does it pervade the larger U.S. bureaucracy? What are the *causal mechanisms* of political control? Does it occur passively through institutional design or actively through manipulation of agency leadership, resources, personnel, or structure? A related concern is the *relative effectiveness* of tools for political control. Is political appointment, budgeting, structure, personnel power, or oversight most important in affecting bureaucratic responsiveness?[1] Rational political control requires knowing how to manipulate the available instruments. Also, agencies may differ in their responsiveness based on certain design criteria. Future policy direction and design would therefore benefit from improved understanding of the characteristics producing bureaucratic stability or responsiveness.

More fundamentally, there are also constitutional issues at stake. If one democratic principal—say, the president—is more effective than others, we might ask whether it is appropriate for policy implementation to reflect executive preferences over those of the Congress or the courts? Some would argue—though certainly not without opposition—that the Congress reflects the public will better than the president because of decentralized constituencies and more frequent elections. In either case, knowing the mechanisms of political control would reveal the relative capacity of democratic institutions to control policy. Such knowledge would allow normative debate of constitutional issues to occur in the light of empirical evidence rather than the darkness of speculation.[2]

Past Research

This study is the first to examine outputs from many different agencies in a comparative context for evaluating political control of the bureaucracy. However, it is not the first to suggest *how* political control occurs. Institutional scholars offer various explanations for political-bureaucratic responsiveness.

Congressional Control

Some congressional scholars assert that legislators are the most important actors to consider, since bureaucrats continuously watch the rewards and sanctions from Congress. However, they suggest that congressional control is difficult to observe since the mechanisms are automatic and indirect (Bendor, Taylor, and Van Gaalen 1987; Calvert, McCubbins, and Weingast 1988; Calvert, Moran, and Weingast 1987). Congress designs agency structure and incentives to assure a relation between legislative preferences and bureaucratic outputs (McCubbins 1985). Additionally, legislators find it more efficient under time constraints to monitor bureaucratic performance indirectly rather than actively through oversight hearings; that is, they rely on program recipients, lobbyists, and interest groups to supply information on agency performance (McCubbins and Schwartz 1984). Should discontinuities occur between legislative preferences and bureaucratic activities, Congress controls resources, legislation, and appointments. Bureaucracies are aware of this and are cautious to avoid alienating legislative principals. Thus, Congress does not have to engage in active and continuous oversight to affect political control. Rather, "anticipative responses" assure that administrative decisions will be consistent with congressional preferences.

Past efforts to model congressional influence confirm the elusiveness of congressional control mechanisms. For example, many studies have considered the effect of committee ideology on bureaucratic performance, with mixed results. Typically, these studies measure ideology as an index derived from ratings by interest groups. Using this measure Weingast and Moran (1983), Chubb (1985), and Moe (1985) found that ideology scores related systematically to hypothesized agency outputs. However, Scholz and Wei (1986) and Eisner and Meier (1990) found no effect from committee ideology.

The measure of ideology used in these studies—interest group ratings—is troublesome for discerning the mechanisms of congressional control. For one thing, these ratings do not reflect specific legislative stimuli but are simply an annual average of past voting records. They are not really even annual, since the scores change most biennially. Thus, ideology scores lack the spatial or temporal resolution to determine how Congress controls the bureaucracy. Additionally, there are collinearity problems associated with congressional ideology scores. When Congress changes, so do the parts of Congress (see, e.g., Cook and Wood 1989). Thus, it is impossible to determine whether bureaucratic responses are due to the entire body, one or more oversight committees, or a multiplicity of forces in the environment of an agency.

Presidential Control

Presidential studies are less nebulous about why bureaucracies respond to the chief executive. According to Moe (1985; see also Nathan 1983; Waterman 1989) the key mechanism of executive control is the appointment and removal power. Modern presidents select political leadership not only for their expertise and to reward supporters but also for their ability to administer the president's plan. The Reagan presidency more than any other epitomized the use of political appointments to affect political control. The Reagan transition team spent months screening those who would serve, emphasizing loyalty and ideology above all other attributes.

Political appointments are important to the modern presidency, but there are also other tools a president can use to achieve political ends. A president through the Office of Management and Budget (OMB) has the initiative in controlling the resources of most agencies. The OMB also monitors the activities of bureaucracies, both their efficiency and their compatibility with the president's program. Through regulatory review (via Executive Orders 12291, 12498, and others) and the Paperwork Reduction Act, the OMB has centralized the rule-making process (Fuchs 1988). While the overall effect of these powers is difficult to assess, severe constraints now obviously exist on the independent policy-making ability of the federal bureaucracy.

The president also affects bureaucratic performance through other channels. Using personnel authority resulting from the Civil Service Reform Act of 1978, the president can choose senior career executives for compatibility with an agenda. The president can use reductions in force or transfers to adjust lower bureaucratic levels. The president can also reorganize a wayward bureaucracy by centralizing control to administration loyalists. All of these powers sum to an unambiguous ability of the chief executive to shape policy.

The multiple tools of the presidency for controlling the bureaucracy can produce a complex pattern of responses through time. Yet past efforts to model presidential effects with empirical methods have been simplistic and unconcerned with capturing this dynamic. The typical analysis has used annual data with dummy variables switching on at the first or second year of a presidency to represent all executive effects (e.g., see Chubb 1985; Eisner and Meier 1990; Scholz and Wei 1986; Scicchitano and Hedge 1989; Stewart and Cromartie 1982). The use of annual data and dummy variables is problematic. Annual data cannot distinguish among the many stimuli that occur early in a presidency. Change can result from appointments, budget changes, reorganizations, congressional effects, or other factors, all occurring at different times. Furthermore, it is incorrect to assume that change in executive influence occurs during only one year of a presidency. Dummy variables so designed may only account for variance, not truly explain it. Clearly, we need a more complex specification with finely divided data to reveal the underlying dynamic of political-bureaucratic relations.[3]

Methods and Data

This study differs from previous research on political-bureaucratic relations in that the designated purpose is to reveal the scope and specific mechanisms of political control. A brief description of our methodology follows (for a full description, see the Appendix). The research employs an explicitly causal logic, considering both the timing and covariation between independent political events and bureaucratic responses. We integrate qualitative methods with quasi-experimental time series analysis. That is, we identify particular events beforehand that should have caused change in bureaucratic outputs by searching archives and/or elite interviews. We define the characteristics of the events including the timing, magnitude, and direction of the stimulus. We then hypothesize that the events caused change in related outputs from public bureaucracies at time t or $t + n$. Box and Tiao's (1975) impact assessment methods are then used to test the hypotheses. Given a statistically significant response from an event, we again use qualitative methods to search for alternative explanations to the observed stimulus-response relationships. The types of events we consider are political appointments, resignations, budget increases and decreases, congressional oversight hearings, administrative reorganizations, legislation, and political signals that should produce "anticipative responses."

Agency outputs include various core regulatory enforcements including litigations, sanctions, and administrative decisions.

The sample of agencies in the analysis consists of the Equal Employment Opportunity Commission (EEOC), the FTC, the Nuclear Regulatory Commission (NRC), the Food and Drug Administration (FDA), the National Highway Traffic Safety Administration (NHTSA), the Office of Surface Mining (OSM), and the Environmental Protection Agency (EPA). We obtained time series of agency outputs through Freedom of Information Act requests. The sample was not random but selected to maximize differences between agencies in organizational design. Three of the agencies are independent regulatory commissions, three are executive department agencies, and one is an independent executive branch agency. The agencies differ considerably in administrative discretion, constituency, issue salience, complexity of implementation technology, and organizational esprit. All of the agencies implement regulatory policies because outputs from such programs are easy to measure. The sample was also shaped somewhat by agencies' willingness to supply data in the form we requested. Still, we can think of no reason why the reported results should not be generalizable to the entire federal bureaucracy.

It is important to emphasize one feature of the data that distinguishes our study from previous research. Unlike most studies, which rely on annual data, our data consist of finely divided time intervals that enable us to explore the underlying dynamic of bureaucratic responses to political events. Most dependent series are monthly observations; but when such data were not available, we relied on quarterly measures. The fine temporal resolution of the data allowed establishing a much closer correspondence between the timing of political events and subsequent bureaucratic responses. In most cases bureaucratic responses occurred in the same month as, or the month following, a political event. We consider this timing to be strong evidence of causal connections between the application of political tools and subsequent changes in bureaucratic performance.[4] We shall now report selected findings to illustrate the scope and mechanisms of political control.

Studies in Political Control

The Equal Employment Opportunity Commission

The EEOC is an independent regulatory commission originally established to administer sections of the Civil Rights Act of 1964 dealing with employment discrimination. That legislation made it illegal for employers to discriminate on the basis of race, color, religion, sex, or national origin. Since 1964 the agency has also received responsibility for implementing the Equal Employment Opportunity Act of 1972, the Equal Pay Act of 1963, the Age Discrimination Act of 1967, parts of the Vocational Rehabilitation Act of 1973, and several executive orders. The primary tasks of the EEOC in these regards are to investigate, conciliate, and litigate complaints of employment discrimination.

Regulating employment discrimination is controversial. For this reason politicians have ample motivation to engage in political manipulation. An examination of EEOC history supports this assertion. In the 22 years from 1965 to 1988 there have been 15 commission chairs and 17 general counsels. The budget and number of personnel at the agency have also varied through time with the changing ideologies of both Congress and the president (Wood 1990). Thus, there are obvious stimuli operating to change EEOC behavior.

Our analysis for the EEOC is constrained by the available data.[5] We report a time series of monthly litigations from 1981 to 1987. Because the data start in January 1981, we consider only stimuli pertinent to the Reagan administration. Still, given the stated preference of the Reagan administration for ending employment quotas and affirmative action, the stimuli after January 1981 should have been especially strong.

The EEOC Office of General Counsel handles litigations. Reagan's nominee to head that office, Michael Connolly, was very controversial. He was an attorney who had previously represented major corporations against the government in antidiscrimination suits. His nomination was vigorously opposed by civil rights groups claiming he would not enforce the law. Still, he was well qualified for the job and confirmed by the Republican Senate in September 1981. Shortly after arriving at the commission, Connolly aroused further attention by telling his staff he would no longer be pressing sexual harassment, age discrimination, equal pay, and class action suits (*Nation*, 8 May 1982). Additionally, Connolly sent directives to the field reversing former policies without discussing them with his own staff or the commission (*Business Week*, 9 August 1982). Connolly's tenure as the EEOC general counsel was controversial, eventually resulting in his resignation in January 1983. His replacement, David Slate, was career-oriented and more palatable to the Congress and EEOC constituencies. Finally, in September 1984 under intense congressional scrutiny, the commission issued a Statement of Enforcement Policy, which declared, "Every case, where violation of an EEOC enforced statute is found and attempts at conciliation have failed, should be submitted to the full Commission for litigation authorization." This effectively removed the final authority of the general counsel over litigation decisions. Thus, we hypothesize three possible stimuli affecting EEOC litigations through this period: the Connolly appointment, the Connolly resignation/Slate appointment, and the commission's Statement of Enforcement Policy.

Table 1 reports the statistical results. Figure 1 contains a graph of the empirical data along with predictions from the model reported in Table 1. The commission's Statement of Enforcement Policy produced no change in the number of litigations. However, the other two interventions were significant. The Connolly appointment produced an immediate decline of 4.57 litigations per month followed by a continued movement to a level 18.28 litigations per month below the preintervention mean. In April 1983, one month after the Slate appointment there was an abrupt increase of 6.58 litigations per month. The Slate appointment simultaneously produced a trend that increased litigations by an average .55 cases filed per month. The lag is reasonable, since it takes longer to initiate litigations than it takes to halt them. Thus, the EEOC initially responded in a completely predictable fashion to the changing philosophy of the presidency. However, executive influence was counterbalanced by Congress and EEOC constituencies, resulting in an appointee who produced movement in a different direction.

The Federal Trade Commission

The FTC, like the EEOC, is an independent commission headed by five commissioners with its chair designated by the president. However, unlike the EEOC, FTC independence has always been a matter of controversy. The agency regulates unfair and deceptive trade practices under the Federal Trade Commission and Clayton Acts of 1914. It also shares responsibility with the Justice Department for regulating unfair competition under the Sherman Anti-Trust Act. The vague

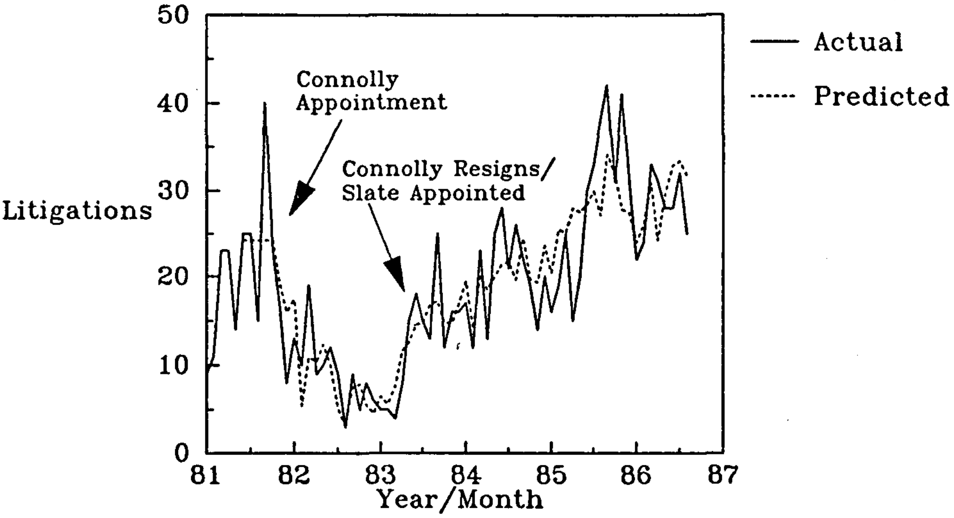
Table 1. The Impact of Political Stimuli on Independent Regulatory Commission Outputs

Variable and Parameter	Equal Employment Opportunity Commission (EEOC)	Federal Trade Commission (FTC)	Nuclear Regulatory Commission (NRC)
Appointment			
ω_0	-4.57	-3.50	-68.41
	(-2.03)	(-2.17)	(-2.02)
δ_1	.75	—	.76
	(6.07)		(4.24)
Midterm resignation/appointment			
ω_0	6.58	—	—
	(2.01)		
ω_0	.55	—	—
	(2.27)		
δ_1	.99	—	—
	(44.54)		
Legislation			
ω_0	—	—	154.82
			(4.09)
ω_0	—	—	214.15
			(5.69)
Noise Components and Diagnostics			
ARIMA	(0,0,1)	(0,1,1)	(1,0,0)
Autoregressive (ϕ)	—	—	.21
			(2.24)
Moving average (Θ)	.51	.81	—
	(3.69)	(15.63)	
Mean (μ)	24.24	—	181.89
	(9.88)		(40.10)
Residual mean square (noise only)	80.69	7.77	1,860.04
Residual mean square (full model)	28.74	7.57	1,404.15
Residual mean square (percent change)	64.00	2.60	24.51
Autocorrelation (Q)	25.59	27.26	15.62
DF	22	23	22

Note: t-ratios are in parentheses. The EEOC and FTC intervention series were step inputs. The NRC intervention series were pulse inputs. The FTC dependent series was differenced to achieve stationarity. The ω_0 parameters show the amount of change in the first time period after an intervention. The δ_1 parameters are a weight reflecting the rate of change in succeeding time periods.

statutory mandate of the agency offers wide discretionary authority to regulators over what sorts of activities to pursue (Katzman 1980a). Thus, from its creation till the 1970s, some argued, the FTC was a quintessential example of agency capture (e.g., Stone 1977). In the early 1970s there was a turnabout at the FTC. A change in staffing produced an agency that vigorously represented consumer interests beginning in 1971 (Meier 1985). In 1977, President Carter's chair, Michael Pertschuk, further increased the consumerism of the agency. In response, business interests launched an effort through Congress to return policy to pre-1970 conditions.

Figure 1. The Impact of the EEOC General Counsel on Litigation Activities by the Commission

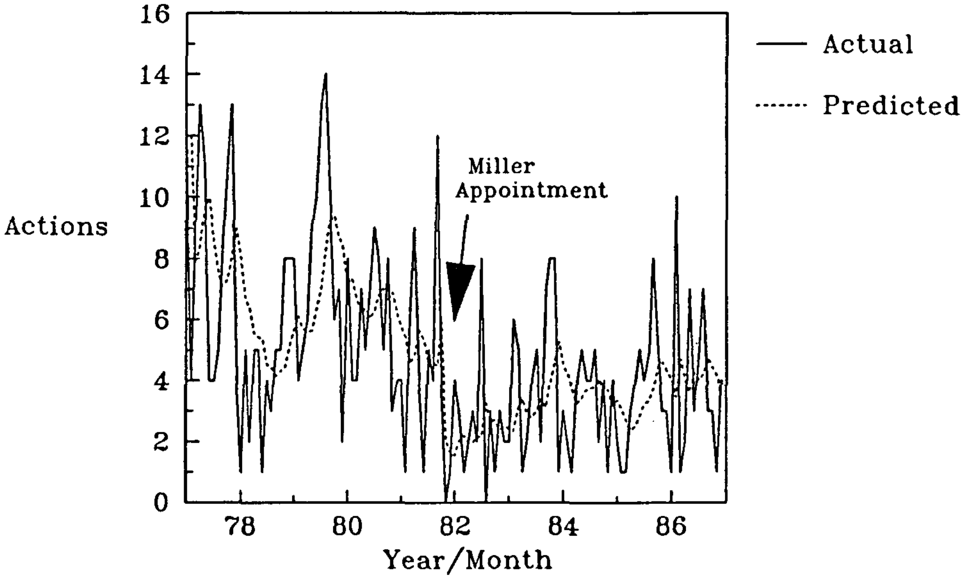


With the Reagan presidency and "regulatory relief," there came the clear expectation that business would dominate the agency. Still, there is considerable uncertainty in the published research over what movements have actually occurred.

For example, Katzman (1980b) considered FTC decisions for 1970–76 on the types of cases pursued. He concluded that internal power struggles within the agency were most important in explaining the mix of cases the FTC handled. In contrast, Weingast and Moran (1983; see also Calvert, Moran, and Weingast 1987) examined annual data for 1964–76 on FTC case selection decisions and found they varied systematically through time with congressional ideology. That is, probusiness congressional orientations meant probusiness FTC case selection. On the other hand, Moe (1982) used annual data for 1945–77 to examine covariations between changing presidential administrations and the number of FTC complaints issued. He concluded that the number of complaints varied through time but not in the manner one would intuitively predict. Republican administrations brought *larger* numbers of FTC complaints against business, an obviously counterintuitive result. In contrast, Stewart and Cromartie (1982) used annual data to examine FTC deceptive trade practice cases and found they vary predictably with changing presidential administrations. However, Yandle (1985) challenged Stewart and Cromartie's finding, arguing that deceptive trade practice cases are only one dimension of FTC performance. The larger picture, he argued, reveals no presidential effects.

Previous empirical research leaves unresolved the question of FTC susceptibility to political control. However, it also suggests some stimuli that should have changed FTC activities if political control really occurs. For one thing there was dramatic turnover in the membership of congressional committees responsible for overseeing the FTC in 1977. Membership changed from a markedly proconsumer orientation before 1977 to a markedly

Figure 2. The Impact of the Chairman on Enforcement Activities of the Federal Trade Commission



probusiness orientation after 1977. Thus, if the agency responded to congressional influence, the vigor of FTC regulation should have declined after 1977. Qualitative evidence suggests the Pertschuk FTC did not yield to congressional influence after 1977, since Congress continued highly visible efforts to manipulate the agency. Between 1977 and 1980 Congress expressed disapproval of FTC policies by not approving an FTC budget and funding its activities through a series of continuing resolutions. In May and June 1980 Congress twice actually allowed funding to expire. A budget-maximizing bureaucracy would have taken these "signals" and changed direction.[6] In late May 1980 Congress passed, and the president signed, the Federal Trade Commission Improvements Act, which authorized FTC funding through fiscal year 1982 but also restricted FTC rule making in certain areas. This may also have affected FTC enforcement. Finally, in July 1981 the Reagan administration nominated James Miller III, a conservative economist, to head the FTC. So the vigor of FTC activities should have declined after Miller's arrival at the FTC in October 1981.

To evaluate whether these stimuli produced responses in FTC behavior, we selected data on the number of enforcements between 1977 and 1987. The enforcement measure was a monthly sum of all consent decrees obtained, final administrative orders issued, and concluding agreements reached by the FTC. The statistical results are presented in Table 1. The empirical data and model-predicted results are presented in Figure 2.

Concerning changing congressional oversight, the budget cutoffs, and the 1980 reform legislation, the data offered no support for the congressional dominance theory. There was no statistically significant post-1977 downward trend. There was also no response to model components for budget cues or legislation. Rather, noise terms explained fluctuations through this period as well as deterministic specifications, so we dropped these terms from the model. Of course, the nonfindings could result from our particular measure, so more work on congressional influence at the FTC is needed.

Concerning presidential influence, however, there was a significant finding. In the same month James Miller III became commission chair there was a drop of about 3.5 (about 50%) in the monthly number of enforcement actions by the agency. Visual inspection of Figure 2 also suggests a continued lower average level of FTC activities after Miller for the remainder of the Reagan administration. However, the time series was drifting, so we could not confidently provide preintervention and postintervention estimates of what the mean levels were. Still, the timing and magnitude of the response to the Miller appointment suggests that the administration appointee affected FTC activities.[7]

The Nuclear Regulatory Commission

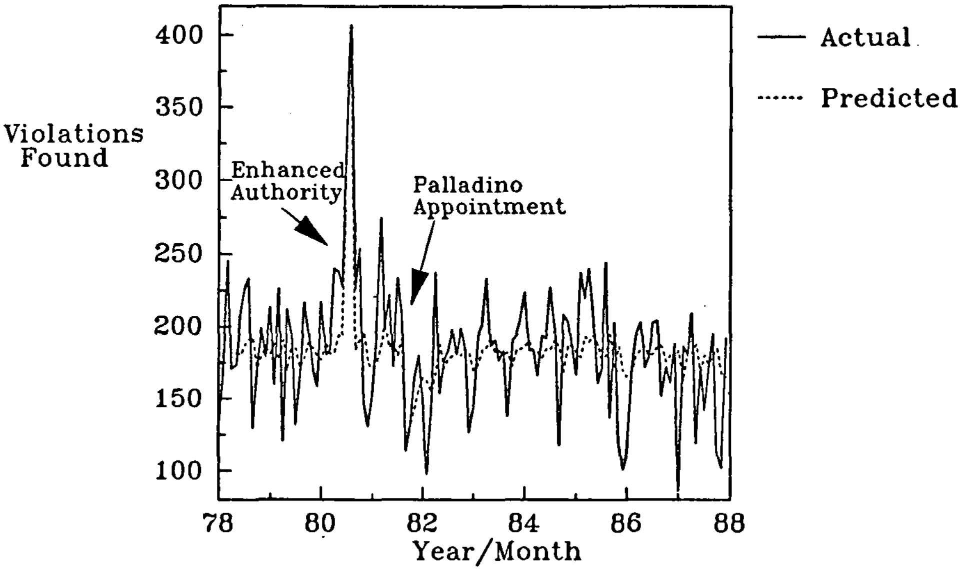
Congress created the NRC in 1974 as a response to suspicions that the older Atomic Energy Commission (AEC) subjugated safety concerns in promoting nuclear power. Congress passed the Energy Reorganization Act of 1974, which split the AEC into two agencies, the NRC and the Energy Research and Development Administration (ERDA). The legislation charged ERDA with developing nuclear and other forms of energy, while the NRC was to regulate nuclear safety. Because of the technological risks associated with nuclear safety, Congress designed the NRC to emphasize technical rationality over responsiveness to political control.

The NRC is an independent commission headed by five commissioners who serve staggered five-year terms. The president designates the commission chair and nominates its membership. However, the president has traditionally exerted little control over agency policy. The OMB does scrutinize the NRC's budget; but it does not review legislative proposals by the NRC, due to the independent posture of the commissioners (Goodman and Wrightson 1987, 166–71). Moreover, the agency's internal structure insulates commissioners from routine administrative functions by separating the commission from its staff (Waterman 1989). An executive director administers the agency. There are also rigid constraints on administrative discretion through very specific administrative rules for dealing with regulatory problems. The purpose is to limit external political influence over NRC field operations.

Because the NRC's organizational design emphasizes technical rationality over external control, the prior expectation is that the agency would be insensitive to political manipulation.[8] To evaluate the expected independence of NRC activities from external politics we selected monthly data for safety violations found by NRC field inspectors from 1978 to 1988. Preliminary interviews with NRC officials suggested two events that may have caused change in NRC enforcements. First, in June 1980 Congress passed legislation increasing the size of the maximum penalty that could be levied by NRC inspectors from five thousand to a hundred thousand dollars per violation. The NRC requested the new legislation, arguing that safety citations had no teeth (U.S., House 1979). Thus, we hypothesized an increase in NRC enforcement as inspectors used their new authority in the months following the legislation.

Second, activities may have responded to changes from the Reagan administration. During the 1980 campaign Reagan expressed the desire to reform nuclear safety regulation and promote the ailing nuclear power industry. Between 1981 and 1987 there was a gradual reduction in the NRC's budget of about 10% in current dollars. According to NRC officials, the budget reductions were almost completely

Figure 3. The Impact of the Reagan Chairman on NRC Enforcement Activity



absorbed by the research program and unlikely to have affected NRC enforcements. A more likely event was the arrival at the commission of Reagan administration chair, Nunzio Palladino. Palladino shared the administration's desire to reform nuclear safety regulation. Accordingly, we hypothesized a decline in NRC enforcements corresponding with the appointment of Nunzio Palladino in June 1981.

Table 1 reports statistical results for these predictions. Figure 3 presents the actual data, along with a graph of the model predictions from Table 1. Both the legislative and presidential interventions were significant. In July and August of 1980 following the new legislation there was a substantial increase in the number of safety violations cited by NRC inspectors. The increase for July was about 155 violations above the preintervention mean, controlling for the effects of autoregressive noise; for August the increase was about 214 additional violations.

We also found a weak, but statistically significant, response beginning in the same month Chair Palladino arrived at the commission. In June 1981 there was an abrupt decline of about 68 violations per month, which decayed gradually back to the preintervention level. This calculates to a total reduction in NRC safety violations of about 285 fewer than would have occurred absent the Reagan appointee. (The total change here is computed $\omega/1 - \delta$). This effect is not large but still demonstrates political influence in an agency that most observers consider to be independent.

The Food and Drug Administration

The FDA, unlike the EEOC, FTC, and NRC, is an executive branch agency in the chain of command of the president. Residing in the Department of Health and Human Services, the FDA has primary responsibility for assuring the safety, purity, cleanliness, and effectiveness of drugs, cosmetics, medical devices, and some foods. In performing these tasks there are wide variations between programs in the discretion granted by Congress. For example, in regulating the market entry of drugs and food additives bureaucratic discretion is tightly bound by the Kefauver-Harris Amendments of 1962 and the Delaney Clause of the Food Additive Amendments of 1958. Congress specified that products must be proven safe and effective before market entry. A rigidly bureaucratized and sometimes lengthy process accompanies this requirement (Quirk 1980). On the other hand, for product withdrawals and regulating the pure and sanitary manufacture of foods and drugs the agency has wide discretion (Meier 1985). Thus, we should see agency responsiveness but variations among programs owing to the varying discretion of administrators. We select one program—regulating the safe and sanitary manufacture of food and drugs—to illustrate the dynamics of political control at the FDA.

In regulating the manufacture of food and drugs, about a thousand FDA inspectors perform on-site inspections of the more than 90 thousand plants located in the United States, as well as import entry points (Greer 1983). On finding violations, enforcement personnel have the full arsenal of administrative tools. They can take informal action by making office visits or phone calls. They can issue formal letters of violation warning the manufacturer of the infraction or possible impending action. They can seek injunctions on the continued manufacture or marketing of unsafe products. They can also ask the manufacturer voluntarily to recall products already marketed. Failing this, they can sometimes seize hazardous products to protect the public health and safety (Bryner 1987; Gottron 1982). Finally, as a last resort, they can file lawsuits against negligent manufacturers or even publicize violations.

We select one of these various enforcement activities—product seizures—to illustrate FDA responsiveness. The data run quarterly from 1977 to 1988. We began with little prior expectation about what events should have changed FDA activities through this period. Reagan's "regulatory relief" agenda may have been important, but food and drug issues were hardly the hot topic of the 1980 campaign. Interestingly, there was a gradual increase in the FDA's budget through the first term of the Reagan administration, so it was not reasonable to expect a response to changing fiscal conditions. On the other hand, the Reagan appointment to head the FDA—Arthur Hull Hayes—was ideologically consistent with Reagan, so the most likely specification was a response to the Hayes appointment.[9]

We report results in Table 2 for FDA product seizures. The empirical data and model predicted results are in Figure 4. As expected, they show a decline in product seizures in the same quarter Commissioner Hayes arrived at the FDA in May 1981. Seizures declined by about 47 per quarter (54%) and remained at this level for the rest of the Reagan administration. Thus, we posit a straightforward case of top agency leadership manipulating the activities of an agency on behalf of the president.

The National Highway Traffic Safety Administration

The NHTSA is situated within the Department of Transportation and regulates automobile safety. An outgrowth of the consumer movement of the early 1970s, Congress charged the agency with establishing "reasonable, practicable, and appropriate" safety standards for automobiles and creating a system for finding and reporting safety defects. The NHTSA conducts its own field tests and engineer-

Table 2. The Impact of Political Stimuli on Executive Department Agency Outputs

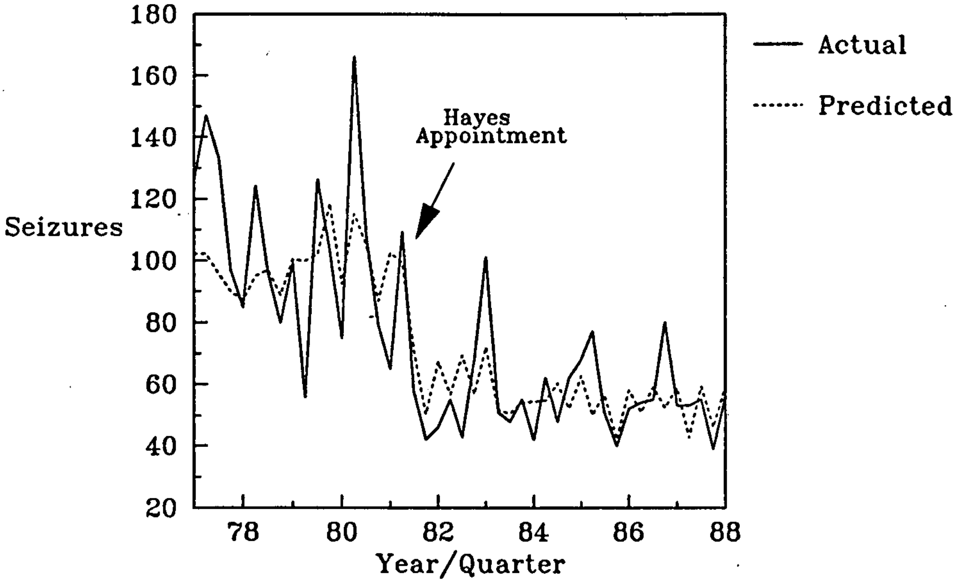
Variable and Parameter	Food and Drug Administration (FDA)	National Highway Traffic Safety Administration (NHTSA)	Office of Surface Mining (OSM)
Early administration appointment			
ω_0	-46.59	20.96	—
	(-15.51)	(3.84)	
δ_1	—	.33	—
		(1.46)	
Midterm resignation/appointment			
ω_0	—	-22.85	—
		(-5.34)	
δ_1	—	.39	—
		(3.56)	
1980 election			
ω_0	—	—	-34.46
			(-5.05)
δ_1	—	—	.75
			(11.77)
Reorganization (ω_{02})	—	—	-24.13
			(-2.10)
Preemption			
ω_0	—	—	26.12
			(2.74)
δ_1	—	—	.59
			(3.36)
Noise Components and Diagnostics			
ARIMA	(1,0,1)	(0,0,0)	(1,0,0) $(1,0,0)_{12}$
Autoregressive (ϕ)	.41	—	.28
	(1.68)		(2.69)
Moving average (Θ)	.83	—	.55
	(4.89)		.28
Mean (μ)	102.20	45.98	—
	(46.88)	(25.50)	
Residual mean square (noise only)	757.23	262.92	274.78
Residual mean square (full model)	387.72	29.33	201.72
Residual mean square (percent change)	48.80	88.84	26.59
Autocorrelation (Q)	10.02	19.99	21.03
DF	21	23	22

Note: t-ratios are in parentheses. All intervention series were step inputs except the NHTSA early administration appointment. The OSM dependent series was differenced to achieve stationarity.

ing evaluations. It also provides a consumer hotline for reporting problems. The agency has legislative authority to fine automakers, but more often relies on voluntary recalls or publicity to affect compliance.

During the Carter administration the NHTSA was a zealous advocate of auto-

Figure 4. The Impact of the Reagan Chairman on Product Seizures by the Food and Drug Administration



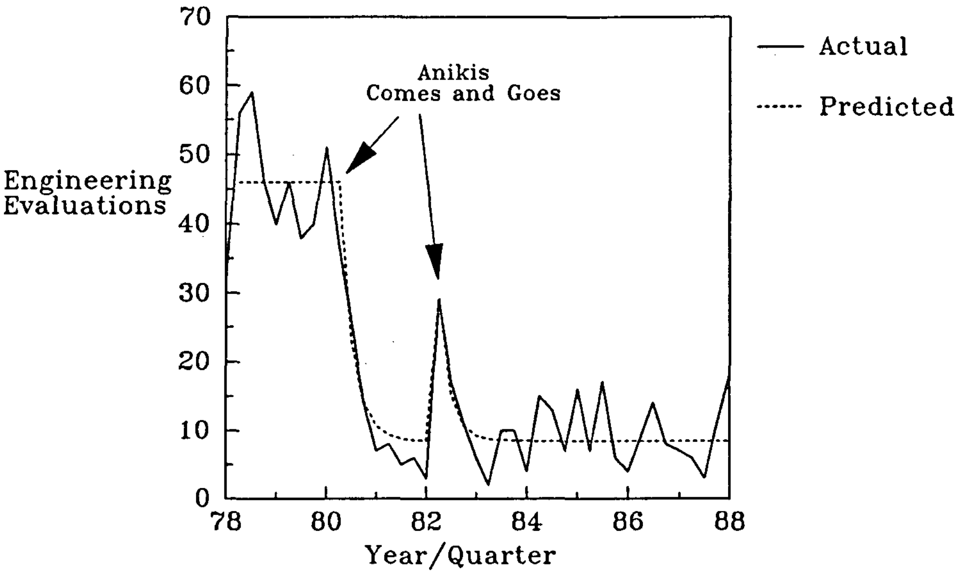
mobile safety standards. Preliminary examination of our data show that Joan Claybrook, the Carter NHTSA administrator, increased agency activities to levels about 16% higher than the Nixon–Ford administration and 19% higher than the Reagan administration. These facts are interesting, though not surprising in light of the cases we have already reported. However, what we found more interesting was that some NHTSA activities declined *before* the Reagan administration, absent any apparent executive or congressional stimulus.

In Figure 5 we graph the data for quarterly engineering evaluations by the NHTSA from 1978 to 1988. The graph shows that the number of evaluations declined as early as the first quarter of 1980, more than a year before any possible effect from the Reagan administration. Furthermore, a search of congressional records showed no apparent inducement from the Congress. Because of the timing and magnitude of this decline, we were compelled to ask *why* such a dramatic change occurred.

We conducted interviews with officials who were at the NHTSA through this period. They directed our attention to a serious staffing error made by Administrator Claybrook. As one official told us, "Claybrook thought she had quite a find." Instead, her appointee proved to be an individual who did not share her regulatory philosophy. George Anikis, according to one respondent "never could find a good reason for conducting an engineering investigation." Thus, in explaining the premature decline we hypothesized that engineering evaluations changed beginning with the Anikis assignment to the Office of Defect Investigations. We also expected to see an increase when he left, given his apparent unpopularity with NHTSA personnel.

Table 2 reports the statistical results. Figure 5 reports empirical data and model

Figure 5. The Impact of Appointments on Engineering Evaluations by the National Highway Traffic Safety Administration



predictions. As expected, the decline in auto safety defect engineering evaluations corresponded perfectly with George Anikis's arrival at the Office of Defect Investigations. In the second quarter of 1980 there was an initial decline of around 23 evaluations. The decline continued until Anikis left in the second quarter of 1982 when the NHTSA conducted about 37 fewer quarterly reviews than the average before his assignment. Interestingly, his departure marked an apparent celebration, since engineering evaluations increased sharply. For one quarter there was a rise of 21 evaluations followed by a rapid decline to the postintervention mean. This level remained for the duration of the Reagan administration.

The Office of Surface Mining

Congress established the OSM within the Department of Interior in 1977 to implement the Surface Mining Control and Reclamation Act of 1977. Before the legislation, regulation of surface mining was a state function. State laws, regulations, and agencies were inadequate, resulting in mine operations destructive of the environment and, in some cases, dangerous to human health. Congress intended to change this by placing primary responsibility for regulation with the national government but through a federal scheme.

The OSM developed rapidly during the Carter administration as an agency intent on obtaining compliance. There was a perceived mandate at the OSM for strong regulation when President Carter publicly expressed disappointment that the legislation was not as tough as he had wanted. This, along with the appointment of several persons of similar philosophy to the OSM start-up task force meant a "gung ho" attitude among agency personnel. Many OSM officials were former state agency inspectors who, disgruntled by their inability to regulate at the state level, wanted to make a point at the federal level (Shover, Clelland, and Lynxwiler 1986). Thus, in 1978, when the agency started operation, there was rapid growth in enforcement activities.

Enforcement personnel used basically five tools to enforce regulatory compliance: inspections, notices of violation, cessation orders, fines, and litigations. Inspections demonstrated regulatory presence. However, many operations, especially those in eastern states, were "outlaw" operations, so that establishing compliance required coercion. A mild form of coercion was the notice of violation warning of impending action. However, in extreme cases when violations continued or there was hazard to public health, safety, or the environment, the OSM issued cessation orders to halt mining operations. During 1979 and 1980 the OSM made liberal use of cessation orders, with as many as 175 written in a single month by the 70 or so agency inspectors.

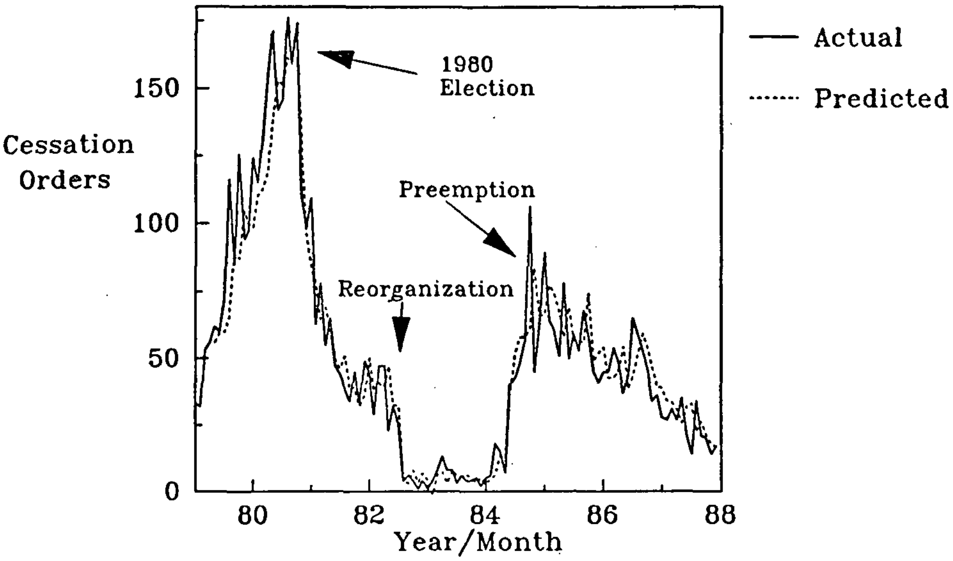
Surface mining enforcement was coercive both to mine operators and to states with economies dependent on mining operations. The OSM personnel did not help relations any by the uncompromising intensity of their efforts. There was also a marked disparity in the vigor of enforcement between eastern and western OSM regions. During 1979 and 1980 OSM inspectors issued about 180 cessation orders in the East for every one issued in the West. Consistently, they also issued about 37 notices of violation in the East for every one issued in the West. The response of eastern operators and states to this apparent discrimination was predictable. An intensive lobbying effort began in Congress to gain relief from the zealots at the OSM.

Some qualitative evidence suggests that the OSM had already seen the wisdom of moderation before the Reagan election in November 1980 (Shover, Clelland, and Lynxwiler 1986, 122). However, if it had not, the early Reagan administration attack on environmental regulation surely made the point. Reagan appointed anticonservationist James Watt to head his Department of the Interior. Watt summarily named James Harris as director of the OSM. In May 1981 Harris informed OSM regional directors the agency was being reorganized. The reorganization plan involved structural changes to facilitate returning "primacy" to the states. It eliminated field enforcement offices. It also reduced the number of OSM personnel from about a thousand to around six hundred because, according to congressional testimony, state agencies would soon be taking over enforcement responsibilities (Culhane 1984; U.S., House 1981; U.S., House 1982). Perhaps most important, the reorganization plan stripped field personnel of discretionary authority to write notices of violation, issue cessation orders, or initiate litigation without prior approval from central offices (Shover, Clelland, and Lynxwiler 1986).

Meanwhile, not all states willingly accepted the "primacy" offered by the Reagan administration. In seven eastern states, judges issued injunctions that prohibited state "primacy" applications. Two states, Tennessee and Oklahoma, refused to provide adequate resources to run their programs according to federal standards. In April 1984 the OSM reimposed federal strip-mining regulation in these two states (Derthick 1987, 70; Pasztor 1984; Reagan 1987, 190). The OSM resumed its preemptive role in prodding state agencies to assume regulatory responsibility.

Several events from this history should have changed OSM behavior. The most obvious is the installation of Reagan administration leadership at the OSM early in 1981. Additionally, there are the reorganization of the agency and removal of enforcement authority from field personnel and the resumption of some enforcement activities in 1984 to prod reluctant

Figure 6. The Impact of the 1980 Election and Reorganization of the Office of Surface Mining on Cessation Orders



states.

To test for these effects we selected a monthly time series from 1979 to 1988 of cessation orders issued by the OSM. Model results are in Table 2. Graphs of the data and model predictions are in Figure 6. Unexpectedly, there was no response to the Reagan appointments. Rather, the decline had already occurred beginning with the Reagan election in November 1980. Follow-up interviews suggest this timing is an example of an "anticipative response" by self-interested bureaucrats trying to save jobs. Agency officials, beleaguered by external pressure, moderated enforcements to appease the incoming Reagan administration. Beginning with the Reagan election in November 1980 there was an immediate reduction of about 34 cessation orders per month. The decline continued to a new level about 138 orders lower than the peak in 1980. By the time Director Harris took control of the OSM, the agency was already subdued. Nevertheless, the new OSM leadership followed through with plans for reorganization. In August 1982 there was a second response. The reorganization produced a decline of about 24 cessation orders per month. Enforcements remained at this level until April 1984. Beginning in this month, the OSM resumed regulation in two reluctant states with an increase of about 26 cessation orders per month. From this level there was a gradual decline in the number of cessation orders to 1988.

The Environmental Protection Agency

President Nixon created the EPA in 1970 through an executive reorganization that consolidated 15 different environmental programs under one administration. The EPA is Washington's largest bureaucracy with an operating budget of $5.3 billion and a staff of over 14 thousand in 1987. The agency implements both regulatory and distributive programs including the Clean Air Acts, Clean Water Acts, sewer grants, hazardous wastes, Superfund, pesticides, toxic substances, marine protection, and noise control.

The EPA's structural arrangement relative to elected institutions is unique compared with the other agencies we have examined. It is an independent executive branch agency with a single administrator located directly in the chain of command of the president. The chief executive appoints the EPA administrator and scrutinizes agency policy through the various control mechanisms of the institutional presidency. However, in spite of the structural advantage to the executive, the courts and Congress have always influenced EPA policy. Most environmental legislation allows citizen lawsuits, ensuring continued involvement of interest groups and federal courts in environmental regulation (Melnick 1983). Congress has also maintained a special relationship with the EPA, since most environmental legislation originated there, rather than in the executive. Through the early years legislative sponsors chaired important oversight committees and held frequent hearings to assure program development. Moreover, congressional attentiveness to environmental issues remained strong due to the high salience and popularity of these programs with the U.S. people. With such a complex array of political forces, it is unclear what particular external stimuli should have affected EPA behavior most. Nonetheless, certain events should have been important.

The saga of the early Reagan administration effort to curtail environmental regulation is detailed elsewhere (Harris and Milkis 1989; Vig and Kraft 1984; Waterman 1989). Essentially, the Reagan administration used the entire array of executive tools to subdue the professional bureaucrats at the EPA. Reagan nominated—and Congress approved in May 1981—a strongly antienvironmental administrator, Ann Gorsuch (later Burford), to head the agency. Before and after Gorsuch's arrival, there were massive personnel shifts and reorganizations of various subunits. Beginning in fiscal year 1982 there were also large budget and personnel reductions for virtually every program. So political control could have come through multiple channels.

With time Congress became disillusioned with administration policy. An aroused EPA constituency, along with the upcoming 1982 elections probably sparked the disillusionment. Hearings began in October 1981 and occurred frequently through July 1982. A specific focus of many hearings was the EPA's management of the hazardous waste program. There was strong evidence that Administrator Burford and hazardous waste chief Rita Lavelle knowingly allowed dumping of hazardous chemicals at illegal sites. Finally, the administration made a crucial mistake. Congress requested hazardous waste enforcement files; but the administration refused to provide them, claiming executive privilege. In December 1982 Congress cited EPA Administrator Burford for contempt of Congress. The ensuing adverse publicity led to the firing of Rita Lavelle and the resignation of all other Reagan appointees. In March 1983 Reagan accepted Administrator Burford's resignation and nominated William Ruckelshaus to replace her. Congress quickly approved the Ruckelshaus nomination, since he was widely respected. Ruckelshaus moved swiftly to restore EPA programs by recruiting zealous personnel and providing strong leadership. In October 1983 Congress reaffirmed its commitment to environmental policy by restoring some of the funds removed in fiscal year 1982.

The preceding suggests multiple stimuli that could and did affect EPA activities from 1981 to 1983. One of us (Wood 1988) reported movements in one EPA

Table 3. The Impact of Political Stimuli on Independent Executive Agency Outputs (EPA)

Variable and Parameter	Estimate	*t*-ratio
Contempt citation (ω_0)	1.34	8.29
FY 1984 budget		
ω_0	.71	3.35
δ_1	.60	5.50
Noise Components and Diagnostics		
ARIMA	$(1,0,0)(0,0,1)_4$	—
Autoregressive (ϕ)	.31	2.86
Moving average (Θ)	.33	3.07
Mean (μ)	1.97	24.90
Residual mean square (noise only)	.28	—
Residual mean square (full model)	.14	—
Residual mean square (percent change)	50.17	—
Autocorrelation of residuals (Q)	18.50	—
DF	21	—

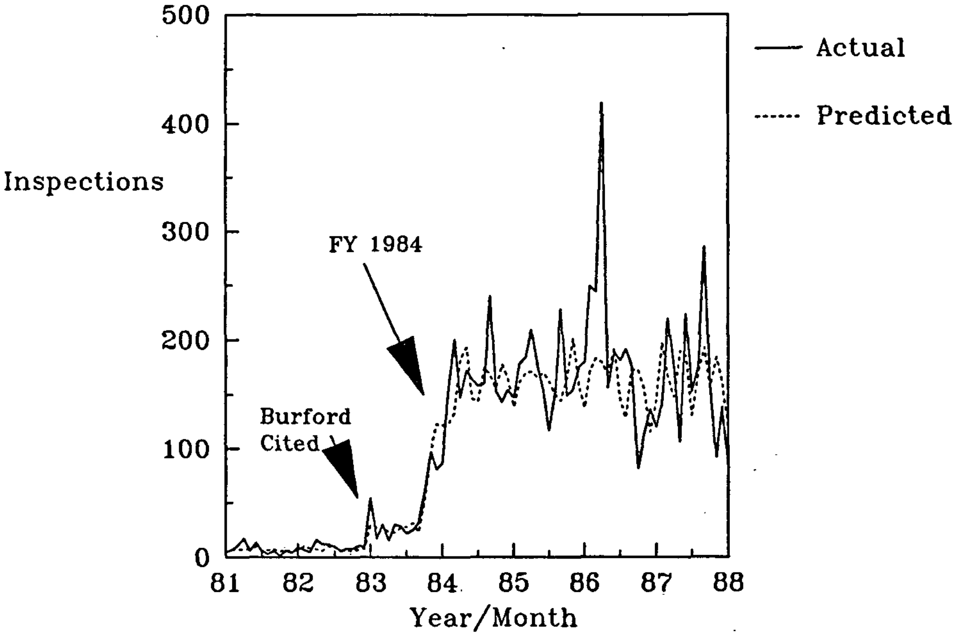
Note: The intervention series were step functions. The series was logged to achieve variance stationarity.

program—clean air—in response to some of these events. That analysis reported no response to the Burford appointment. However, there were strong responses to the fiscal year 1982 budget reduction and the Burford resignation/Ruckelshaus nomination. Wood's analysis (1988) showed clearly that there were limits on executive power over the bureaucracy. However, it does not capture the full dynamic of policy movements at the EPA through this period, since it employed an aggregate measure of only a single program. Moreover, that analysis involved a program ancillary to the central policy dispute between the president and Congress.

We report here an analysis for the EPA's hazardous waste program, which was the focus of the controversy between the president and Congress. Table 3 and Figure 7 describe the fluctuations in hazardous waste inspections between 1981 and 1988.[10] We found no statistically significant response to any of the early Reagan administration stimuli reported elsewhere (Wood 1988). However, the hazardous waste program was not firmly established by 1981, so the Reagan administration effect was probably to delay program development rather than to suppress already-established programs. However, we did find responses to later events in this scenario.

Beginning in January 1983, one month after Congress cited Administrator Burford for contempt, there was a step increase in hazardous waste inspections by regional offices. This was two months before the resignation of Administrator Burford and the nomination of William Ruckelshaus to head the EPA. Numerically, the contempt citation caused a change in inspections to a level 3.82 times higher than the preintervention mean (or from about 7.17 to about 27 inspections per month). However, inspections are a resource-intensive activity, so larger increases were not possible until Congress restored EPA's budget. Beginning in fiscal year 1984 there was an immediate doubling of the number of inspections, followed by a gradual increase to a new level 5.96 times higher than the level after the Burford contempt citation (or from about 27 inspections to about 163 inspections per month). Thus, for the EPA policy in which Congress was most directly in-

Figure 7. The Impact of the Burford Contempt Citation and Fiscal Year 1984 Budget on EPA Hazardous Waste Inspections



volved, legislative influence was clearly manifest through the powers of oversight and appropriations.[11]

Implications and Conclusions

These case studies demonstrate causal movements in the programs of seven different federal bureaucracies from the late 1970's through most of the 1980s. Viewed in isolation, each case study contributes something to understanding the particular policies discussed. However, considered as a unit they offer improved understanding of the dynamics of public policy control more generally. We think this second view is the more important to emphasize for several reasons. First, the seven cases reveal the scope and mechanisms of political control of the bureaucracy. Second, they suggest some of the covariates of bureaucratic responsiveness that may guide future research on political control. Finally, the case studies point to a form of policy analysis that could enhance future democratic control of the bureaucracy.

Concerning the scope of political control, we found causal movements in all seven of the agencies we examined. Movements occurred repeatedly through time for some agencies. Stated more generally, at certain times in the cycle of U.S. politics, responsiveness permeates the U.S. bureaucracy. As new democratic majorities sweep newly elected officials into office, nonelective bureaucracies transmit demands for change. Of course, the election of Ronald Reagan and a Republican Senate in 1980 marked an extraordinary time in the cycles of U.S. politics, so the stimuli to bureaucracies were very strong. Nevertheless, the fact that movement occurred in all seven cases suggests responsiveness is not all that uncommon.

Concerning the specific mechanisms of political control, the case studies demonstrate that political appointment—a shared tool of the president and Congress—is very important. In five of the seven programs we examined, agency outputs shifted immediately after a change in agency leadership. In four of these cases (the NRC, the EEOC, the FTC, and the FDA) change followed an appointment at the beginning of a presidential administration. The direction and magnitude of these responses reflects the increased power of a chief executive in the period after a presidential election. However, the case of the EEOC also shows change in the middle of an administration when pluralist forces are more likely to dominate. In both cases, the responses reveal that political appointments dominate the dynamic of institutional control.

However, one other case we studied—the NHTSA—highlights the importance of political appointments beyond the context of institutional control. The Anikis appointment at the NHTSA reflected neither congressional nor presidential influence, since no Senate confirmation occurred and the change was an apparent mistake by the Carter administration. In this unusual situation, movement followed the path prescribed by the appointee. The implication is that political appointees, even without top-down support, can have extraordinary influence over agency policy. This case also suggests the efficacy to elected officials of careful selection processes and diligent monitoring of agency activities.

The leadership of an agency is the most frequent mechanism for changing agency behavior. However, it is not the only mechanism. Reorganizations, congressional oversight, and budgeting are also important. The OSM case illustrates the importance of agency organization to political responsiveness. When leadership at the OSM and Department of the Interior centralized control of enforcement decisions, there was an immediate reduction in enforcements by field personnel. Thus, the principle is, *The greater the centralization of agency decision-making processes, the greater the executive control over bureaucratic outputs.*

The EPA's hazardous waste enforcement program illustrates the importance of congressional oversight and budgeting to political control. Enforcements by the EPA's hazardous waste compliance division increased sharply after intense congressional interest and the resulting Burford contempt citation. However, the contempt citation was largely symbolic; and the agency could not respond fully without increased enforcement capacity. It took the budget increases of fiscal year 1984 to move hazardous waste enforcements to significantly higher levels. Thus, congressional signals were important; but it was the congressional appropriations process that determined EPA's capacity to respond to political demands.

Our expressed purpose was to reveal the scope and mechanisms of political control of the bureaucracy. We accomplished this task with findings so rich in detail that full exposition is difficult here. However, there are also implications for future research that should be discussed. We have always known that the stable equilibria of bureaucracies reflect passive political control by the Congress and the president, who jointly create federal programs, as well as by the courts. However, the pattern of movements shown above also demonstrates active political control. We believe this evidence for active political control is so strong that controversy should now end over *whether* political control occurs (see, e.g., Eisner and Meier 1990; Rourke 1989). Future research should turn toward exploring the *determinants* of political control.

Several questions should be important

to future research on the determinants of political control. Does responsiveness depend on bureaucratic structure, personnel attributes, mission complexity, or administrative constraints? How do constituency effects relate to political-bureaucratic equilibria? Is issue salience an important factor? This study by design, with its focus on discrete stimuli, was not equipped to answer such questions. However, comparison of movements between agencies does imply something of these relations.

On the one hand, agency responsiveness and stability can roughly be arrayed along a continuum which aligns nicely with certain bureaucratic attributes. The agencies most responsive to executive influence, gauged by the magnitude and duration of change, were those situated in the executive departments. The FDA in the Department of Health and Human Services, the NHTSA in the Department of Transportation, and the OSM in the Department of Interior had outputs that remained lower than their preintervention levels for the duration of their series. These agencies also implemented programs lower in issue salience, weaker in constituency, and further from congressional attention.

On the other hand, the agencies with the most stable outputs were the independent regulatory commissions. The NRC reflects the extreme position since its outputs moved only briefly away from an equilibrium to return quickly without external intervention. The EEOC Office of General Counsel responded for a time to executive influence, but outputs rebounded when Congress and aroused constituencies intervened. The FTC was the single exception to this rule, since its outputs remained depressed throughout the Reagan administration. However, the FTC was also exceptional in that both the president and Congress agreed that decreased FTC activity was desirable. Thus, structure is important—but not overriding when multiple democratic principals jointly demand a policy outcome.

Findings about the mechanisms, scope, and covariates of bureaucratic responsiveness are important because they increase scientific knowledge of U.S. democracy. However, the findings and methods used here also suggest a form of policy analysis that could enhance future democratic control of the bureaucracy. We believe that analyses of the sort presented here, which we dub *policy monitoring,* should become routine for federal agencies. Federal agencies should report selected outputs into a central tracking system on a regular basis, where they would be available to policy analysts and academics. Analysts would then evaluate outputs, making them available to institutional actors and the public. Such analyses would serve the scholarly purpose of tracking movements in the pluralist equilibria of the U.S. system. They would also serve a social purpose in making politicians and bureaucrats more accountable and informed.

Knowledge by the policy community of consequences tied to specific policy actions (e.g., nominations or confirmations) would focus attention on public officials, making them more responsible for their actions. Potentially, this could alter the incentive structure of institutional processes. Of course, some public officials might prefer avoiding the increased scrutiny policy monitoring could bring, so the method may never be formalized by government. However, whether monitoring is done by government officials or academics, such activity would benefit in other ways.

Policy monitoring would offset some of the information asymmetry that now exists between the president, the Congress, and the bureaucracy. The empirical literature cited earlier suggests that elected representatives seldom engage in effective oversight of agency activities. This works to the advantage of actors with the most information (i.e., bureaucrats or politicians who would conceal policy movements). The solution to this problem of democratic control is increased information. Thus, policy monitoring would allow public policy change to occur in the full view of all concerned participants. It would also allow rational feedback systems to develop for correcting unwanted movements away from a pluralist equilibrium.

Of course, policy monitoring is not a panacea. Some bureaucratic outputs are nonquantifiable or difficult to analyze. Under monitoring there could also be a tendency for some agencies to focus on outputs, rather than outcomes (i.e., bean counting). Data-reporting systems could be subject to misuse by unscrupulous agencies or individuals. However, we believe the potential benefits of policy monitoring far outweigh the limitations. Increased information could only open up the political process and remove the deep-seated suspicions that now surround nonelective bureaucracies in a representative U.S. democratic system.

Appendix: Methodology

The goal of the research was to find the "best" model for explaining political control at each agency we studied. This involved hypothesis testing, but not in the traditional one-shot, deductive sense. Rather, it involved dynamic iterative procedures that integrated both qualitative and quantitative methods.

We did an initial case study of each agency to develop working hypotheses. The purpose was to determine what particular stimuli could possibly have caused political control at an agency. After all, it made little sense to hypothesize that political appointments caused change when an appointment was nonideological. Likewise, one could hardly assert that budgets, reorganizations, congressional oversight, or other political tools changed bureaucratic outputs when these stimuli were not present. Thus, we searched archives for contextual evidence on possible causes of changed bureaucratic outputs.

This qualitative research supplied initial expectations about what particular stimuli could possibly have caused change at the various agencies. It also told us of the timing, magnitude, and duration of potential changes. With this information, we formulated hypotheses for time series analysis of monthly or quarterly data. The independent series were dummy variables designed to operationalize predictions from the case studies.

We modeled several dependent series for most agencies. Each reflected a different dimension of the agency's "core technology" output as defined by Thompson (1967). However, we have reported only one measure for each agency because of space limitations. We selected models to report based on clarity of presentation and robustness of results.

We used Box-Jenkins (1976; see also Box and Tiao 1975) methods to do the time series modeling. These methods were well suited to the task because of their unique ability to establish causal relations with finely divided data. Box-Jenkins methods establish causal sequence through empirical evidence (i.e., the cross-correlation function and hypothesis tests), not just by a priori theorizing. Another strength of these methods is their overriding concern with controlling extraneous historical effects. They require the user to develop dynamic forecasts of current observations based on past observations (i.e., ARIMA models). These dynamic forecasts are then included in final models to control for background noise before hypothesis testing.

Another reason we chose Box and Jenkins's techniques was their consistency with our research strategy. We viewed modeling as a *process*, not just a one-shot hypothesis-testing procedure. Some conflict was anticipated between case study results and statistical analysis. Consistently, Box-Jenkins methods portray modeling as an iterative process that continues until a best model results (1976, 17–19; McCleary and Hay 1980, 141–45). The researcher uses empirical tools to define and redefine model specifications until satisfying some objective statistical criterion.

The objective statistical criterion we used in this study was the minimum of Akaike's Information Criterion (AIC). We chose AIC because it is "a mathematical formulation of the principle of parsimony in model building" (Akaike 1974, 719). It measures the goodness of fit while imposing a penalty for overspecification (Sawa 1978). The statistic is computed $AIC = 2\ln(L) + 2k$, where L is the likelihood function, and k is the number of free parameters. We emphasize that AIC will never minimize where a model retains extraneous information, so the final models we have reported contain only relevant variables.

In some instances the model-building process involved iteration between qualitative and statistical methods. This was because some of the initial hypotheses from case studies were rejected by the statistical analysis. In these instances we did follow up research by reexamining archival sources and conducting interviews with officials. Based on new information, we developed second-iteration hypotheses. Statistical analysis began again. The process continued until we obtained a best-fitting model for each series.

We should note that this research design provided two different types of control for alternative explanations. First, it provided statistical control by modeling the history of each time series as an ARIMA process, which assured that random chance, seasonal fluctuations, and omitted process variables did not cause the findings. Second, it provided quasi-experimental control by integrating case study materials with time series analysis. Campbell and Stanley observe that the major threat to validity for the time series design is history (1963, 39; see also Cook and Campbell 1979, 211). That is, some event other than the hypothesized event may have caused the change. To check this possibility we did follow-up case study analysis to verify the consistency of the statistical findings with reality.

We should also emphasize that multiple tests of the same stimulus-response relationships greatly increase the strength of our design. Five of the seven agencies under investigation showed a response in the month or quarter after a political appointment. Two of these agencies showed responses to multiple appointments. These appointments all occurred at different times. Finding a plausible alternative explanation for all of these occurrences seems highly implausible.

Notes

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1. The term *responsiveness* used here and throughout means the extent to which outputs from a bureaucracy change with application of an external political stimulus. Accordingly, we view political appointments, changing budgets, reorganizations, oversight activities, personnel manipulation, and other events as potential stimuli for change.

2. We would not presume here to decide what the role of the executive and legislatures should be with respect to political control of the bureaucracy. This is a normative question that cannot be settled by empirical research. Moreover, those issues are best left to the courts or political philosophers. Still, empirical research can offer evidence as to the consequences of political control of the bureaucracy by various constitutional actors.

3. There are two exceptions to the methodological critique offered here. Moe (1985) used quarterly data to evaluate outputs from the NLRB. In creating dummy variables for separate presidencies, he assumed that change occurred in the quarter after the first political appointment. This was by assump-

tion, however, and Moe was more concerned with *who* affected policy than *how* they affected it. On the other hand, Wood (1988) was concerned with the mechanisms of political-bureaucratic control. Monthly data were used to evaluate responses in EPA's clean air program to various political stimuli. Still, that study involved only one agency, so it may not be generalizable.

4. We should also point to a disadvantage in using finely divided data. Some process variables (e.g., political ideology or interest group influence) do not lend themselves well to such analysis because there are no monthly measures of these factors. Thus, this analysis is restricted to how events, not processes, affect bureaucracies. Certainly, not all political control mechanisms operate instantaneously, as we have pointed out elsewhere (Wood and Waterman 1990). Nevertheless, since many are applied abruptly, we should still see many of the mechanisms of political control.

5. For the EEOC we had only one measure of outputs that had finely divided time intervals—litigations. The series we report includes litigations for individual cases of employment discrimination, and gender discrimination litigations into one measure. The EEOC was uncooperative in providing additional monthly data.

6. Ferejohn and Shipan (1989) argue the importance of congressional "signals" in their case study of the Federal Communications Commission (FCC). They observe that Congress acted in a cohesive and rational fashion to change FCC decisions on user fees after the deregulation of the telephone industry.

7. We should note here that the FTC suffered budget reductions in March and October 1981 from the Carter proposed budgets for fiscal years 1981 and 1982. There was no perceptible response to the May reduction. Unfortunately the October reduction coincided precisely with Miller's arrival at the commission, so it is difficult to say with complete confidence that the observed response did not have a fiscal component. We opt for the appointment explanation because the FTC did not respond to the earlier fiscal interventions. However, over the longer term, budgets may have been important.

8. Moreover, Goodman and Wrightson assert, based on a qualitative assessment, that the Reagan administration was unsuccessful in manipulating the NRC's regulatory process through either appointments or the budget (1987, 178–81). So a finding of responsiveness would be interesting.

9. Technically, the FDA commissioner was appointed by the secretary of the Department of Health and Human Services, Richard Schweiker, not by Ronald Reagan. Senate confirmation of the appointment was not required.

10. Some data were available before 1981. However, EPA officials cast doubt on their validity because the hazardous waste reporting system was not fully in place.

11. We can confirm the response to the Burford contempt citation with four other measures, including litigations, notices of violation, informal actions, and formal administrative orders. The estimates reported for the hazardous waste series in Table 3 are in the natural log metric. A log transformation was necessary to equalize preintervention and postintervention variances. The ratios reported here are computed $\exp^{\omega o}$. The mean levels are simply antilogs of the sums of reported statistics.

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STREET-LEVEL POLITICAL CONTROLS OVER FEDERAL BUREAUCRACY

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Local partisan activities of legislators and their electoral coalitions systematically influence field office activities of federal bureaucracies in their electoral districts. This alternative to centralized democratic controls over bureaucracy occurs because discretionary policy decisions made at the field office level are influenced by local resources generated through partisan activities. Our study of county-level Occupational Safety and Health Administration enforcement in New York (1976–85) finds that county, state, and federal elected officials influence local enforcement activities, with liberal, Democratic legislators associated with more active enforcement. The county political parties are most influential for activities with the most local discretion, while members of Congress are more influential for local activities more readily controlled by the national office.

Few political scientists would argue with the proposition that politics has a tremendous impact on the behavior of government bureaucracies. There is less agreement, however, on exactly how politics influences bureaucratic behavior and whether the influence is good or bad. Democratic theorists argue that elected officials (president and Congress) should and actually do determine the general direction of bureaucratic behavior both through legislation and through a variety of centralized controls over bureaucracy (Fiorina and Noll 1978; McCubbins and Schwartz 1979; Moe 1982, 1985; Weingast and Moran 1983). Administrative and policy analysts, on the other hand, emphasize that local politics often divert policies from the goals desired by central leadership. Some analysts emphasize random, idiosyncratic disturbances from local politics (Bardach 1977; Derthick 1972; Pressman and Wildavsky 1984). Others emphasize systematic diversions of policy goals by organized interests, particularly those that can "capture" administrative agencies by operating at both central and local levels (Bernstein 1955; Huntington 1952; Selznick 1966). With few exceptions (e.g., Berman 1978; Elmore 1979) local political influence is generally seen as a harmful "diversion" from policies controlled by central democratic institutions and as a (perhaps inevitable) source of bureaucratic inefficiency (Pressman and Wildavsky 1984; Seidman 1980).

We shall argue that some kinds of political influences at the local interface be-

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tween bureaucracy and society can reinforce democratic controls over federal bureaucracies. The primary argument is that partisan activities of elected officials and their electoral coalitions in the local arena provide important systematic influences on bureaucratic behavior, particularly in circumstances in which conflict reduces the ability of central institutions to exercise political control. We emphasize the role of nonlegislative or "homestyle" activities of legislators and their support coalitions in electoral districts of local, state, and federal legislatures. These activities include the broader interaction of elected officials with other local elites as well as the more frequently studied ombudsman activities for individual constituents (Cain, Ferejohn, and Fiorina 1987; Fenno 1978). More specifically, we argue that federal bureaucracies behave predictably differently in electoral districts with Democratic and Republican representatives, whether these be local, state or congressional electoral districts.

Many studies have described the formal and informal mechanisms by which the president and Congress influence federal agencies and have demonstrated their effectiveness controlling the discretion granted to agency leaders to implement and enforce legislative statutes. We extend this literature by focusing explicitly on the problem of bureaucratic discretion at the "street level" (Lipsky 1980) and on informal controls by elected officials that influence local rather than central discretion.

The local influence of elected officials on federal bureaucracies has not been subjected to the kind of systematic empirical study that has been done for central influences. Jones (1981, 1985) has demonstrated that local partisanship affects metropolitan bureaucracies, but this influence may work through normal hierarchical channels at the level of the director of the local agency rather than through the street-level bureaucrat. Previous studies have demonstrated the influence of state-level partisan politics on federal bureaucracies (Chubb 1985; Scholz and Wei 1986). However, Chubb argued explicitly that influence derives from Washington-based activities of representatives; and Scholz and Wei's research design could not rule out central activities as the source of partisan influences. Furthermore, Hedge, Menzel, and Williams (1988) argued that federal officials enforcing strip-mining regulations were resistant to local partisan influences, at least in comparison with state enforcement agencies.

To provide a more decisive test for the influence of local partisan activities, we focus on county-level activity of the federal bureaucracy. While state representatives and governors might be argued to have sufficient national influence to exert controls through presidential and congressional channels, county-level party organizations and elected officials are unlikely to have sufficient means of influencing federal bureaucracies through central controls. This is particularly true given the irregular (i.e., gerrymandered) jurisdictional boundaries of various electoral districts, which crisscross counties in different ways for state and federal legislatures. Evidence that partisan differences affect enforcement at the county level would considerably strengthen the argument that it is the local activities associated with partisan politics that directly influence field office behavior of federal bureaucracies.

Our discussion begins with a description of federal bureaucracies as loosely coupled systems in which field offices inevitably have considerable discretion in following statutory and administrative policy guidelines. We focus particularly on regulatory enforcement and on the manner in which partisan activities in the field office environment influence discretionary choices about the stringency of enforcement. We consider specifically the case of the Occupational Health and Safety Administration (OSHA), where the partisan identity of the two most interested groups—Republicans for business and Democrats for labor—is relatively well defined. We then present our analysis of county-level OSHA enforcement in New York State from 1976 to 1985. The magnitude and robustness of local party influence on federal enforcement strongly support the hypothesis of local partisan influence. On the other hand, the even greater effect of the Reagan administration provides an appropriate perspective on the relative importance of central and local influences. Finally, the pattern of congressional influence suggests that representatives influence bureaucracy through both local and central activities, with local influence becoming more important for activities with greater degrees of field office discretion.

Political Controls and Field Office Discretion

The Weberian image of a rational, centrally controlled bureaucracy continues to dominate political analyses of federal bureaucracy even though it has been replaced in organizational theory by a recognition of the more open structure and broader range of environmental influences affecting most complex organizations (Perrow 1986; Scott 1987). To cope with the complexity of the environment in which organizations like federal agencies operate, Simon (1962) has argued that organizations develop specialized subunits to deal relatively independently with different parts of their environment. Organizational charts of federal agencies thus provide a diagram of important parts of the agency's environment that are dealt with through specialized, semiautonomous offices. Essentially, agencies like OSHA have developed a national office with multiple specialized functions (budgeting, personnel, rule-making functions, legal services) interacting primarily with their counterparts in other organizations in the central environment (Washington) and a number of field offices where the "production work" takes place, all interacting with local clientele, local elected representatives, and local officials from other government agencies. Our primary concern here will be the field offices.

Sources of Field Office Discretion

This image of bureaucracy helps clarify the sources and consequences of field office discretion. Just as legislative statutes inevitably provide considerable discretion to bureaucratic leaders, so, too, do policy guidelines from the agency's national office provide discretion to officials in the field offices. In adopting national guidelines to the specific local environment they face, street-level bureaucrats can exert considerable influence on policy outcomes (Lipsky 1980; Pressman and Wildavsky 1984).

The problem of controlling field office discretion is common to all federal bureaucracies. For example, Kaufman summarized factors affecting the way rangers working at the field level in the forest service use their discretion: "The customs and standards of the group [rangers] work with, the values and attitudes and pressures of the communities in which they reside, and the preferences and prejudices they bring with them from their extra-organizational experiences and associations may lead them in a variety of directions" (1960, 87).

Although the problem is a general one, our discussion will focus on regulatory enforcement as practiced by OSHA. Its field office discretion arises from the generic problem of interpreting national regulations under a diversity of changing local conditions (Bardach and Kagan

1982; Diver 1980; Hawkins 1984). Observers of regulatory enforcement consistently comment on the broad discretion that each inspector must deal with in determining how closely to scrutinize a given establishment, whether observed conditions constitute a violation, and whether a violation is intentional and should be cited or "accidental" and should be dealt with informally (Hawkins 1984; Kagan 1989). Discretion leads to considerable variation in the way different inspectors (Feinstein 1989) and field offices (Hedge, Menzel, and Williams 1988; Hutter 1988; Shover et al. 1984) enforce the same law and even to variation in the way similar cases are treated in the same office (Kagan 1978).

Field office discretion may develop in directions undesired by central authorities and their political superiors for three reasons: lack of knowledge, inability, or refusal to do what the central office wants (Kaufman 1973). These themes have been developed concretely in several studies. In the first case, internal bureaucratic factors such as local decision rules, operating procedures, and personal interests of bureaucrats fill the vacuum when central guidelines are ambiguous (Levi, Meltzner, and Wildavsky 1974; Lipsky 1980). In the second case, resources required by the field office to complete statutory tasks may be controlled by local groups, who can exchange resources for more favorable interpretations of central guidelines (Pressman and Wildavsky 1984; Selznick 1966). As noted in organization theory (Pfeffer and Salancik 1978; Scott 1987), local influences will increase in importance as resources obtained from the central environment (budgets, authorized procedures) decrease in importance in comparison to resources obtained from the local environment (cooperation, local assistance).

In the third case, local bureaucrats may sympathize with local groups and share interpretations of policy guidelines favorable to those groups (Blau 1963; Kaufman 1960). This congruence in outlook is particularly problematic for regulatory enforcement officials, who spend much of their time in contact with businesses they regulate, frequently share similarities of professional outlook with their counterparts in firms, and face considerable psychological pressure to reduce the confrontational nature of their daily contacts with the businesses they inspect (Blau 1963; Kelman 1981; but see Quirk 1981).

Control Strategies of National Offices

Of course, federal agencies counteract local influences with a range of techniques and strategies, which include procedures for controlling and "performing" decisions, detecting and discouraging deviation, and developing the will and capacity of field officers to conform to central guidelines (Kaufman 1960). OSHA and all federal agencies employ similar techniques to reduce local influences on field office discretion.

First, enforcement activities in particular are governed by thick manuals in response to judicial demands for due process and consistent treatment (Pierce, Shapiro, and Verkuil 1985). Compliance officers spend much of their time documenting completed inspections in a manner that justifies their actions according to official guidelines. The procedures are probably more completely specified than is the case for most other federal agencies not only because of stringent administrative law requirements governing enforcement procedures in all regulatory agencies but also because OSHA enforcement decisions can be appealed to a special review commission that was set up in part to restrict enforcement activities (Moe 1989).

Second, communication and computer technology has considerably increased information available to monitor and detect deviations from normal patterns of field office and even individual officer behavior. Every federal inspection is recorded in OSHA's management information system. The central office is awash with information about field office activities, although the interest and ability of central leaders to make sense out of this mass of information is less than certain, as Kaufman (1973) noted of six other federal agencies.

Third, OSHA attempts to recruit compliance officers who have had professional training as safety engineers and industrial hygienists. Furthermore, in-service training programs help develop common beliefs about appropriate enforcement behavior. However, these techniques to shape common beliefs and loyalties appear to be considerably less effective for OSHA than Kaufman (1960) found them to be for the forest service in the late 1950s. Hiring and retaining qualified compliance officers has become a constant problem because of uncompetitive wages, civil service restrictions, and the hostility of the Reagan administration toward OSHA enforcement.

Political Polarization Limits Central Controls

A critical limitation on OSHA's control of field office activities derives from the polarized central political environment in which it must survive. From the beginning, business, labor, and their respective allies clashed on many issues directly relevant to enforcement (Moe 1989; Noble 1986). Labor won the critical battle to place OSHA within the Department of Labor rather than Commerce and also won the right of workers to file complaints alleging OSHA violations in their plant. As a consequence, OSHA's level of enforcement has been strongly influenced by the willingness of workers in a given area to file such complaints (Headrick 1990; Scholz and Wei 1986). But business won the battle over the independence of the review commission, which has allowed businesses to influence the level of enforcement by threatening to appeal more citations; appeals could reduce the level of enforcement by diverting inspection resources to these judicial proceedings (Headrick 1990).

Political polarization has continued to plague OSHA's national office. Its oversight committees have been used as examples of extremely partisan committees (Fenno 1973) that produce much confrontation, occasional truces, and little consensus. As a consequence, OSHA officials note with some frustration that they are probably enforcing at the appropriate level when both business and labor are upset with them. While political conflict encourages the profusion of formal procedures that bureaucrats can use to protect themselves, it also undermines the national office's willingness and ability to provide clear, unambiguous signals to field offices about how vigorously to pursue formal procedures.

Changes in presidential administration have further weakened central controls over field offices. Carter's administration attempted to shift enforcement priorities to the most hazardous plants and most serious violations, while Reagan's administration emphasized consultation and tried to minimize enforcement. These contrasting views led to changes in signals from the central office, and undoubtedly exacerbated the usual divergence in beliefs among field officials about how stringently to enforce regulations (Kelman 1981; Noble 1986). Budget cuts and morale problems under the Reagan administration further exacerbated general civil service problems of hiring and maintaining qualified compliance officers. These changes have most affected the ability of OSHA's national office to shape common beliefs and loyalties among compliance officers, leaving them more open to influences from the communities in which they live and work.

In summary, enforcement activities inevitably leave considerable discretion to field offices to determine how vigorously regulations should be enforced. National offices have powerful techniques for channeling discretion in directions preferred by the central political institutions. But for OSHA, local influences have been enhanced by statutory provisions for local participation, which provide business and labor groups with enforcement resources important to field offices. Furthermore, the polarized, dynamic political environment has lead to reluctance and inability of the central office to provide field offices with clear, legitimate guidelines for desired levels of enforcement. While OSHA's procedural guidelines and monitoring system provide powerful standardizing and integrating influences, the problematic political environment has worked against the development of standardized beliefs and values among field officials about the appropriate level of enforcement.

Political Influences over Field Offices

Our primary focus is on the influence of local electoral politics on field office enforcement activities. Particularly in polarized policy arenas such as OSHA's, conflicts among central political authorities lead not only to more delegation to the bureaucracy (McCubbins 1985) but also to more delegation within the bureaucracy to the field office. We argue that field office responsiveness to local electoral politics reintroduces a measure of local democratic control, which compensates for the failure of central institutions to provide sufficient democratic controls at the national level.

Specifically, the empirical study analyzes the impact of local electoral politics on OSHA enforcement activities at the county level.

Hypothesis 1. *Countries with more liberal, Democratic representatives in federal, state, and local elected offices will have higher levels of enforcement than counties with more conservative, Republican representatives.*

Three different explanations are consistent with this relationship: local influences may work through the home-style activities of elected representatives in their own districts (Fenno 1978); the coalitions responsible for electing representatives may also be responsible for direct influences on bureaucracy; and the general political culture in a given community may determine both who gets elected and how the bureaucracy behaves. All three are consistent with the notion of local democratic controls over bureaucracy, although each points to different views about the leadership role of representatives and the importance of elections in controlling government actions.

Home-style Leadership Influences on Enforcement

The first explanation highlights both the ombudsman and leadership role of elected representatives in the local community. Congressional studies in particular have noted the growing electoral importance of providing constituents with help in dealing with federal bureaucracies (Cain, Ferejohn, and Fiorina 1987; Fenno 1978). Although studies of congressional constituency service generally argue that members of Congress also provide such services for individuals outside the member's electoral coalition (Cover and Brumberg 1982; Fenno 1978; Fiorina 1977; Fiorina and Noll 1978; Mayhew 1974; Serra 1989), at least one study has found that service requests come primarily from the representative's party (Johannes 1984).

There have been few attempts to evaluate the impact of congressional constituency service on patterns of bureaucratic behavior. However, Jones's (1985) study of building code enforcement in one city found local party influence on enforcement that operated through a comparable mechanism—the party's ability to bring complaints to the notice of the enforcement agency. City officials armed with budgetary and other oversight powers forwarded complaints to the agency from wards with strong party organizations. These wards consequently received the higher levels of enforcement they desired.

While such direct case-level interaction with bureaucracy is likely to influence field office behavior for service-oriented agencies, it is less likely to do so for federal enforcement agencies. Court-imposed administrative norms generally forbid elected officials interested in a case under adjudication to meet with the enforcement official handling the case, although the role of Congress has not been fully clarified (Pierce, Shapiro, and Verkuil 1985). Furthermore, labor and business have less need for ombudsman help with OSHA, since they can complain directly to OSHA or appeal its decisions. It is therefore not surprising that participant observation yields little evidence of direct case-by-case influence by politicians on OSHA enforcement (Headrick 1990).

Consequently, the leadership rather than the ombudsman role of elected representatives appears more likely to influence OSHA enforcement. A broader set of constituency activities such as speaking engagements and participation in various district-level meetings may be of greater significance for federal agencies. This local play of partisan politics may provide a largely unrecognized democratic influence over federal bureaucracy by affecting two of the three concerns of central control: the resources needed for enforcement and the beliefs and loyalties of field office officials.

Resources. Consider first the impact on resources. Local leadership activities of representatives may influence OSHA enforcement efforts through the provision or denial of negotiating resources needed for these enforcement tasks. Political leaders can meet with local businesses to iron out OSHA-related problems, thereby minimizing enforcement resources tied up in a special problem. They can help coordinate efforts of unions, local health officials, trade associations, and OSHA to tackle local health and safety problems or to crack down on particularly troublesome employers. Alternatively, the official could be a catalyst for efforts by business to tie up OSHA enforcement by requiring warrants for all inspections and by appealing all citations.

Such leadership activities may have a broader effect on the perceived legitimacy of the agency's actions among local groups important to the agency, which in turn affects the options available to the agency's field officers (Lipsky 1980; Nakamura and Smallwood 1980; Selznick 1966). For OSHA inspectors such responses may change the boundaries within which they can operate without concern for complaints about overzealousness on the one hand or complacency on the other (Bardach and Kagan 1982). Supportive norms provide easier access to firm sites, more useful information about firm conditions in response to compliance officer inquiries, better ability to target problem firms for inspections, and less time and effort involved in documenting violations for appeal proceedings. Given the considerable importance of local business and union responses for OSHA enforcement, effective local support from elected officials may provide important resources for the field office.

Field Office Beliefs and Loyalty. Local leadership activities can also directly affect policy preferences of bureaucrats by changing their beliefs and loyalties. In particular, actions that legitimate the agency's mission in the local community may reinforce agency morale and thus invigorate enforcement (Bardach and Kagan 1982; Blau 1963; Kelman 1981). The inevitable confrontations inspectors face in the enforcement process impose high psychological costs. Without some countervailing esprit de corps and belief in the mission of the agency, enforcement efforts are difficult to sustain on a daily basis (Blau 1963; Shover et al. 1984). Budget cuts adversely affecting enforcement programs and the hostility of the Reagan administration toward regulation limited the ability of OSHA's national headquarters to foster enthusiasm in field offices. Consequently, recognition of the importance of their task by local political leaders and support groups may have been particularly important for OSHA to provide the psychological boost compliance officers need to maintain a high level of enforcement.

In sum, constituency activities of representatives may influence enforcement through their effect on resources needed by the field office and on the beliefs and loyalty of compliance officers. The field office will find it easier to enforce more rigorously in communities where liberal Democratic representatives effectively influence the community, and are also more likely to heed the representative's demands for more enforcement activity. Alternatively, enforcement will be more difficult when conservative Republican representatives predominate, and the field office is less likely to go against demands for more moderate levels of enforcement.

Electoral Coalition Influences on Enforcement

It is often difficult to distinguish between leadership and passive representation, given the problem of determining motivations and the causal direction of effects. For example, does a conservative representative giving inflammatory speeches lead business to resist OSHA enforcement, or does the electoral importance of resistant businesses cause the representative to make conservative speeches? The difficulty is increased (but also made less important) by shared beliefs and values frequently observed between representatives and their primary constituency of hard-core supporters. Fenno, for instance, provides several examples of members of Congress who feel most at home with business groups or organized labor groups in their district, not infrequently because these groups initially recruited the member to run (1978, 18–24).

But how would electoral coalitions influence OSHA without the leadership of the elected representative? Sabatier (1988) argued that representatives act primarily as brokers for successful "advocacy coalitions" that form around specialized policy areas in their districts. Interest groups who join advocacy coalitions develop shared beliefs and values and pursue common strategies to obtain desired policies. Thus, local groups concerned with OSHA would act to influence both the elected representative and field office activities, creating a spurious relationship between representatives and bureaucratic activities. The coalition would influence enforcement in the same way representatives would—by influencing resources as well as beliefs and loyalties. The congruence of beliefs and values may be particularly important to the extent that field officers themselves play a role in the evolution of the relevant advocacy coalition (Sabatier 1975). Further highlighting the importance of beliefs and values, one study of bureaucratic responsiveness (Gormley, Hoadley, and Williams 1983) actually used the congruence of beliefs and values between citizen and business groups on the one hand and public utility commissioners and staff on the other as the measure of bureaucratic responsiveness.

It is not critical for our argument about democratic influences to distinguish between these explanations: bureaucracies would be responsive to democratic controls whether they are influenced by the representative or by the coalitions that elect the representative. Both explanations imply that elections at the local, state, and federal levels independently affect field office activities. But implications differ about the relative responsiveness of field offices, which can provide suggestive evidence about the relative strength of the two explanations.

The leadership explanation implies that representatives most active and accessible to constituents in the local arena should have the greatest impact on county-level OSHA enforcement. It seems plausible that county-level representatives are most active and most numerous, state representatives somewhat less numerous and less active because of the time and energy spent in the state capital, and members of Congress the least numerous and least active locally because of their full-time position in Washington and the larger size of their districts. This would suggest that the magnitude of impacts should follow in the same order.

Hypothesis 2. *Enforcement actions subject to local discretion will be most responsive to local representatives, somewhat responsive to state representatives, and less responsive to congressional representatives.*

The electoral coalition argument would presumably not predict such differences, since coalition activities are primarily local at all levels of elections and would not necessarily reflect hierarchical differences in activity. Thus, evidence that influences differ as predicted would provide some support for the leadership explanation.

Another implication of the leadership hypothesis is that members of Congress have more direct access to the national office than members of state or local legislatures and therefore should have relatively more influence over national office decisions.

To the extent that certain field office activities are subject to greater control by the national office, the leadership hypothesis would presumably claim that congressional controls should be strongest for these activities.

Hypothesis 3. *Enforcement actions most readily controlled by agency headquarters will be most responsive to congressional influence in comparison with other legislative levels.*

Political Culture's Influence on Enforcement

The final explanation suggests that local areas have relatively homogeneous political cultures that influence both elections and bureaucratic operations in the area. Presumably, the shared beliefs and values within the local community would extend to both the representative and bureaucrats serving in the local field office and would direct discretionary enforcement decisions along the lines preferred by the local population.

This explanation is distinguished from the electoral explanation primarily by the assumption of homogeneity. Instead of a dynamic system in which coalitions of business and labor interests compete with each other in elections at each tier in the federalist system, this explanation implies a relatively stable system in which elections in a given area should exhibit considerable uniformity at each federalist level. Thus, a uniform measure of the political culture in a county should be the best predictor of enforcement in that county.

Hypothesis 4. *Enforcement actions will be more responsive to a single, aggregated measure of local political influence than to the individual measures for* *representatives at various tiers of the federalist system.*

The Study

We analyzed the relationship between local partisan activities and OSHA enforcement activities for 30 counties during the period 1976–1985. Counties in a single state under direct OSHA enforcement (New York) were selected to control for potential state-level differences; New York counties contain a sufficient diversity of political conditions to provide an adequate test of partisan differences.

Data

Enforcement Variables. Our dependent variables consisted of three annual measures of enforcement for each county: the number of inspections, the number of inspections with penalty (which we will refer to as the number of penalties), and the amount of penalty. These three measures have been frequently employed in studies of the effectiveness of enforcement. All 30 counties with significant levels of OSHA enforcement activities (defined as more than .5% of the state total during the period) were included in the sample. In addition, the measures were obtained separately for OSHA activities in manufacturing (SIC 20–39) and construction (SIC 15–17), the two main industrial categories that account for more than 80% of OSHA enforcement actions.

We used several measures and subsets of enforcement actions in order to establish the reliability and generalizability of our findings. In addition, the different measures and subsets provided two dimensions for comparing political effects for different levels of local discretion. The first contrasted two kinds of enforcement activities (inspections and penalties) while the second contrasted enforcement actions in two industrial sectors (manufacturing and construction).

The first comparison assumed that local discretion was greater for activities relating to penalties than for the number of inspections. Although OSHA's enforcement guidelines in fact provided detailed instructions for determining penalty amounts for citations, individual inspectors in most agencies have been found to differ in how they apply these criteria to a given set of circumstances (Hawkins 1984). The national office can target the number of inspections a field office is expected to perform based on observable characteristics of the office's jurisdiction, such as the number of inspectors, the dispersion of work sites, and the complexity of plants within the office's jurisdiction. On the other hand, assigning targets for the frequency and amount of penalties to be imposed is much more difficult, since penalties depend on the relative levels of compliance, which are difficult for the central office to determine and may vary considerably across jurisdictions. The national office cannot easily distinguish more stringent enforcement in a district with compliant firms from less stringent enforcement in a district with less compliant firms, since both could produce a relatively low number of penalties.

The national office has considerable incentive to utilize its controls over inspections to satisfy political demands, since the number of inspections come under considerable scrutiny in oversight hearings. Scholz and Wei (1986) found that the number of inspections were very responsive to political influences and argued that inspections provide a convenient symbolic method of satisfying political demands. More inspections can be accomplished with the same number of compliance officers by simply decreasing the time per inspection, while more penalties require more total time to document citations and handle increased levels of appeals. Although penalties have been found to be more important than inspections for decreasing workplace injury rates (Scholz and Gray 1990), inspections can be more readily controlled and altered than penalties.

The second comparison assumes that enforcement in the construction industry is more difficult for the national office to standardize and monitor than in the manufacturing industry (Rees 1988). Safety problems at construction work sites change constantly. Specific companies are difficult to locate, and subcontracting of construction tasks complicates the assignment of responsibility for accidents and violations to particular companies. Building practices vary according to local requirements. In general, construction trade associations are less supportive of OSHA than manufacturing unions. Since local OSHA officials must adapt to varied and changing local conditions, central influences are likely to be less influential in construction. In manufacturing, on the other hand, work site locations are fixed and owned by a single entity, production processes are generally standardized, unions are more cooperative, and industries are better organized with fewer marginal firms. Such conditions allow the national office to provide standardized guidelines that can be more readily monitored. Since local discretion should be greater in the construction industry, we can compare the influence of local political controls on construction and manufacturing enforcement actions to test the hypotheses about local discretion.

Political Variables. Our independent variables included the political measures of central importance as well as economic and interest group proxies that served primarily to control for possible spurious effects. We gathered measures of party affiliation and legislative ratings along the liberal-conservative dimension where these were available for representatives at each electoral level of the federalist system. We used the legislative ratings from the AFL-CIO's Committee on Political Education (COPE) for members of the House of Representatives and ratings from the New York State chapter of the American Civil Liberties Union (NYCLU) for members of the New York State Assembly and the New York State Senate. Since party and ideology measures were highly correlated and represented the same partisan proclivity toward OSHA that is of interest in our hypotheses, they were combined to form a single partisan score for each elected official. Legislative ratings received equal weight with party affiliation in calculating a partisan score for each elected official that ranged from -1 for conservative Republicans to 1 for liberal Democrats.[1]

Since legislative districts did not correspond to county boundaries, we next calculated an overall index of partisan activities for each county. The index apportioned to each county the proportion of time and effort a representative was assumed to spend in that county, based on the proportion of the representative's electorate residing in the county.[2] This proportion was then multiplied by the partisan score to provide the relative balance of "full-time equivalent" Democrats and Republican legislators working in county. For example, if a county contained one full district and half of a second and both elected liberal Democrats with partisan scores of 1, the county score would be 1.5; if both representatives were conservative Republicans with partisan scores of -1, the county score would be -1.5; and if the legislator from the full district were a Democrat and the half-time legislator a Republican, the county score would be .5.

No such adjustments were necessary for partisan representation in local and county government, where access to data provided the greatest challenge. Given differences in elected positions across counties, a simple count of Democrats and Republicans elected in local elections would be difficult to collect and compare across counties. Using the number of county votes cast in presidential or other elections would be confounded by personality and other campaign issues. Consequently, we adopted a measure considered by scholars of New York politics to be a good indicator of the relative dominance of Democrats or Republicans in county elections (Scarrow 1983). Personalities and issues played less of a role than basic party loyalty in elections for the New York State Supreme Court (the *lowest* court in the New York judicial system), so that they have been considered to be a good indicator of local party strength. Our local partisan measure calculated the difference in votes cast for Democrats and Republican party candidates as a proportion of the total vote for Democrats and Republicans in the county's Supreme Court elections.

Finally, the change in presidential administration from Carter to Reagan was represented by a dummy variable equal to one for the years of the Reagan administration and zero otherwise. This variable was included primarily to control for the major policy, personnel, and budgetary changes affecting OSHA that were associated with the Reagan administration (Noble 1986). The New York governor and U.S. senators were not represented, since there was, of course, no variance across counties (which provides our primary concern) and little variation over time that could be represented in our analysis. All variables except for the presidential dummy were scored so that a more liberal score or a party affiliation of Democrat was the high end of the scale. Thus, we expect positive coefficients for all political variables except Reagan.

Control Variables. Previous OSHA studies have found enforcement activities to be responsive to enforcement conditions (primarily unemployment and injury rates) and interest group activities (Headrick 1990; Marlow 1982; Marvel 1982; Scholz and Wei 1986). To reduce the threat that significant effects observed for the political variables of interest in this study were due to spurious effects of variables not included in the estimation, task and interest group proxies were added to the data set. Per capita income was used to measure the relative wealth of different counties, while unemployment was used to reflect the economic stress in the county. Both may influence the trade-off inevitably facing enforcement officials between costs (in jobs and wages) and safety benefits, with higher per capita income increasing enforcement levels and unemployment decreasing them.

Three measures reflected both the relative size of the county and the relative strength of business and labor influences. The number of employees determined at least the upper limit of labor strength. Similarly, the number of firms determined the upper limit of business actors, and the industry payroll provided a proxy for the financial resources available to businesses in the county. Thus, we expected the number of employees to be positively related to enforcement and the other two measures to be negatively related. It could be argued, of course, that these measures reflect both business and labor strength in a way that cannot be readily untangled. We could not come up with ratios or factors that incontrovertibly distinguished business from labor strength and therefore chose to enter these variables independently but place little emphasis on the interpretation of their coefficients as representing clear interest group influences. They remain primarily control variables. Other measures of union strength in counties (including the annual strikedays times the number of workers affected per county) were gathered but were dropped because the data appeared to be unreliable and the variables were insignificant in any case. Separate data were gathered for manufacturing and construction industries for analysis of these subsamples.

Estimation Techniques

To answer our question about the influence of partisan politics on county-level enforcement, the basic equation we estimated with our data set was

$$\begin{aligned}\text{enforcement}_{i,t} &= a_i + \beta_1\text{local party}_{i,t-1}\\ &+ \beta_2\text{assembly}_{i,t-1}\\ &+ \beta_3\text{state senate}_{i,t-1}\\ &+ \beta_4\text{Congress}_{i,t-1} + \beta_5\text{Reagan}_{i,t-1}\\ &+ \Sigma\beta_{6j}\text{control}_j + u_{i,t},\end{aligned}$$

where i represents the counties, t represents the calendar year, j represents the five control variables, a_i represents the intercept for each county, and u is the error term. We have assumed a one-year lag between enforcement and the five political variables (as in Scholz and Wei 1986). The local influence hypothesis will be supported if the coefficients for the political variables (β_1–β_4) are positive and significant, particularly for the lower levels of elected officials.

Several procedures were employed to minimize the problems associated with regression estimations when cross-sectional and time series data are pooled (Sayrs 1989; Stimson 1985). Goldfeld-Quandt tests confirmed our suspicion that the difference in size between counties introduced considerable heteroscedasticity, with the variance in all variables in the larger counties being larger than the variance in smaller counties. The logged form of all enforcement and control variables except unemployment reduced the difference in variance between large and small counties. Unemployment was not logged because it measured a ratio that was independent of the size of the county.

The political variables posed a different problem of heteroscedasticity. The measure for local party needed no adjustment, since it represented a ratio independent of county size. Initially, we divided the other political measures by the number of "full-time equivalent" representatives in the county to obtain a comparable partisanship ratio ranging between −1 and 1 for each county. Further analysis revealed the opposite problem with this ratio; larger counties had *smaller* variance than the smaller counties, presumably since a change of one representative created a much greater change in a small county. Scatterplots also revealed consistent differences for small and large counties in the relationship between this ratio and enforcement variables. Both problems were eliminated by developing an adjustment function based on the number of representatives in the county that minimized the difference in variance between the counties.[3]

We employed a fixed-effects model to further control against spurious effects from important factors we could not measure (e.g., the relative hazardousness of county industries as measured by injury rates). To control for such unmeasured differences between counties that were stable over the period of analysis, individual intercept terms (a_i) were included for each county.[4] We should note that the fixed-effects model presumably would give conservative estimates of coefficients in that effects of stable cross-sectional political differences may be absorbed by the county intercept terms.

Since Goldfeld-Quandt tests indicated some remaining problem with heteroscedasticity, Parks's (1967) method of estimation was used for the primary estimations presented in Table 1. This method provided efficient treatment for heteroscedasticity introduced by contemporaneous correlation between cross-sectional units. It also corrected for the first-order autoregressive error diagnosed by pooled Durbin-Watson tests.

To test the results for robustness, we

Table 1. The Impact of County-level Political Influences on OSHA Enforcement

Variable	Inspections	Inspections with Penalty	Amount of Penalty
Reagan	-.35** (16.15)	-.34** (9.96)	-.44** (33.53)
Congress	.21** (10.51)	.15** (47.71)	.11** (14.25)
State senate	-.01 (1.25)	.05** (93.27)	.01** (3.97)
State assembly	.14** (9.10)	.06** (9.67)	-.01** (3.43)
Local parties	-.003 (.27)	.17** (25.28)	.18** (47.36)
Number of employees	-.004 (.07)	.17** (12.67)	.02** (3.83)
Number of firms	-.08* (1.99)	.007 (.52)	-.001 (.08)
Payroll	-.19 (1.68)	-.45** (22.97)	.06* (2.41)
Unemployment	.03** (3.21)	.03** (6.59)	-.06** (11.93)
Per capita income	.20 (1.62)	.57** (14.43)	.08** (4.63)
Constant	.15** (14.70)	.19** (11.83)	.52** (31.73)

Note: Entries are standardized regression coefficients on county-level data estimated with the Parks technique and fixed effect model (mean of each county set to zero for all continuous variables). Annual data includes 30 New York counties for the 10 years from 1976 to 1985. The absolute value of *t*-ratios are reported in parentheses.

$^{*}p < .05$.

$^{**}p < .01$.

also estimated the equation with ordinary least squares for the amount of penalty (where the Durbin–Watson test indicated only a marginal autoregressive problem) and with a one-period autoregressive model for inspections and frequency of penalty. Furthermore, we replicated each analysis individually on the manufacturing and construction data sets to check the stability of coefficients on these subsamples. These more-standard regression equations explained considerable variance for fixed-effects models, with R-squareds ranging from .33 to .51 for the manufacturing sector and .10 to .20 for construction and the total data set. The R-squared is less meaningful for the generalized least squares estimation used in Table 1 and was not calculated. The results of the 18 estimations (three dependent variables, three samples, and two models) are summarized in Table 2 for the political variables of interest. They confirm the overall robustness and replicability of the results reported in Table 1.

Results

Hypothesis 1: Multiple Political Impacts on Enforcement

The results reported in Table 1 support the first hypothesis that political influences from all levels of the federalist system affect OSHA enforcement activities at the county level. The Reagan administration exerts the largest and most consistent influence across all enforcement measures, with the Reagan impact being consistently larger. The strong negative effect of Reagan on county-level enforcement indicates an impressive strength of central control even at the county level. Note that the Reagan variable, unlike all other variables, does not vary from county to county and therefore can only explain variance in enforcement over time. The single most important effect during the period of our study can thus be traced to the federal level.

Congress also exerts strong, consistent

Table 2. Robustness and Reliability of Political Effects

	Political Variables				
Dependent Variable	Reagan	Congress	Local Party	State Assembly	State Senate
Inspections with penalty					
Manufacturing					
Autoregression	n.s.	+	+	n.s.	n.s.
Parks	–	+	+	+	n.s.
Construction					
Autoregression	–	n.s.	+	n.s.	n.s.
Parks	–	+	+	+	+
Total					
Autoregression	–	+	+	n.s.	n.s.
Parks	–	+	+	+	+
Amount of penalty					
Manufacturing					
OLS	–	+	+	n.s.	n.s.
Parks	–	+	+	n.s.	n.s.
Construction					
OLS	–	n.s.	+	n.s.	n.s.
Parks	–	+	+	+	(–)
Total					
OLS	–	+	+	n.s.	n.s.
Parks	–	+	+	(–)	+
Inspections					
Manufacturing					
Autoregression	–	+	n.s.	n.s.	n.s.
Parks	–	+	n.s.	+	(–)
Construction					
Autoregression	n.s.	n.s.	n.s.	n.s.	n.s.
Parks	–	+	(–)	+	+
Total					
Autoregression	–	+	n.s.	n.s.	n.s.
Parks	–	+	n.s.	+	+
Number of coefficients significant in expected direction	16	15	12	7	5
Percentage of coefficients significant in expected direction	89	83	67	39	28

Note: Entries indicate the sign of significant coefficients ($p < .05$) for all political variables in estimations for each dependent variable using the indicated data subsamples and estimation models. Each row represents a separate estimation. All variables in Table 1 have been included but not reported in each estimation. Signs in unexpected directions are reported in parentheses. Insignificant coefficients are reported as n.s.

influence over enforcement; counties with liberal, Democratic members of Congress have significantly higher levels of enforcement. While this influence might be exerted either through central or local political arenas, the influence of the other three levels is most clearly related to the local levels. Members of the state senate and assembly significantly affect enforcement in the expected positive direction for four of the six coefficients, although the effects are much smaller than for Congress or Reagan. Only the impact of assembly members on inspections is comparable in magnitude. The significant negative impact of assembly members is puzzling, but at least it is very small in magnitude.

The most conclusive evidence of strictly local influence comes from the significant impact of local party strength on two of the three enforcement measures. Counties in which the Democratic party has greater strength in local elections have considerably more and higher penalties than counties in which Republicans are stronger, even after controlling for other political and economic influences. Furthermore, this influence is of a comparable or greater magnitude than the influence from Congress over these two activities.

To give an idea of the impact of these changes on enforcement measures, an increase of one standard deviation in an independent variable with a coefficient of .10 would signify a 6% increase in annual number of penalties from the mean county value of 26 to 28.[5] The same increase would raise the amount of penalty by 13%, from the county mean of $7,500 to $8,400 and would raise the number of inspections by 5% from the county mean of 79 to 83. A one-deviation change in local party strength from political neutrality in the Democratic direction, for example, would raise the number of penalties in the average county from 26 to 29 and the amount of penalties from $7,500 to $9,300.

The other variables in Table 1 are included primarily to control for spurious effects and will not be discussed in detail. The number of employees is positively related to the frequency and amount of penalties, consistent with its use as a proxy for worker strength. The number of firms has a significant impact on inspections in the expected negative direction, as did payroll on the number of penalty inspections. As a proxy for business strength, payroll's positive impact on the amount of penalty is unexpected but relatively small. The county's wealth, as reflected in per capita income, is positively related to the number and amount of inspections. This is consistent with the view that concern with safety increases with income, leading to greater demand for enforcement in richer counties. Unemployment negatively affects the amount of penalty, consistent with previous analyses (Scholz and Wei 1986). Unemployment's positive effects on inspections and number of penalties with inspections are unexpected, perhaps due to measurement problems on the county level, where commuting across county lines to work is relatively common. Unemployment is measured for residents, and not for the work force, which should influence enforcement. The interpretation of these variables as appropriate proxies may be controversial. We argue only that their performance justifies inclusion to control for economic and interest group variations among counties.

The results presented in Table 2 confirm the robustness and replicability of these findings across two different model specifications, three different dependent variables, and two different subsamples of the data. The variables representing both Reagan and Congress are the best performers, being significant ($p < .05$) for 16 and 15 coefficients estimations, respectively, out of the total of 18. The local party variable is next, having 12 significant coefficients. The results for state representatives are less consistent, being significant in seven of nine Parks estimations for the state assembly and only five of nine for the state senate. The claim of significance for both variables thus depends on the appropriateness of the Parks specification, since they are not significant in any ordinary least squares or autoregressive models.[6] We will discuss the pattern of results in considering the other hypotheses.

Hypotheses 2 and 3: Home-Style Leadership Versus Electoral Coalitions

The pattern of findings in Tables 1 and 2 provide some indirect evidence that the home-style leadership activities of representatives rather than activities of their electoral coalitions influence local enforcement actions. The two hypotheses that distinguish between leadership and coalition activity explanations compare results involving different levels of local discretion: for enforcement measures, penalty-related activities have more local discretion than inspections; for industries, enforcement activities in construction have more discretion than in manufacturing.

The second hypothesis implies that the local party will be most influential on penalty-related activities and the construction industry, where ubiquitous local party activities would be more important than the more sporadic local activities of members of Congress. The third hypothesis implies that congressional controls would be greatest on inspections and manufacturing, where the member's access to the national office might be used to exert influence on the field office over and above the direct influence from the member's local activities.

The results for the Congress and local party variables provide support for the leadership explanation in both hypotheses. Local party has a strong, consistent impact on the frequency and amount of penalty but little impact on inspections. Furthermore, the magnitude of standardized coefficients in Table 1 indicate that the local party has more influence than all other representatives on the state or congressional level for the number and amount of penalties. Local party is significant in every estimation of frequency and amount of penalty in Table 2 but insignificant in all estimations for inspections.

Congress, on the other hand, exerts a greater influence on inspections than on the number or frequency of penalties, according to the magnitude of coefficients in Table 1. Congressional controls appear to be of the greatest magnitude over the more centrally controlled inspections, although congressional influence is equally significant across all three enforcement measures in Table 2.

The comparison of manufacturing and construction in Table 2 suggests that Congress has a less robust influence than local party over construction, the industrial sector providing the greatest discretion to field officers. The congressional variable is not significant for any enforcement measure in the construction industry for ordinary least squares and autoregressive estimates but is significant for all enforcement measures in manufacturing for all estimates. The local party variable, on the other hand, influences both industries equally. Although this finding may just reflect model misspecification for the non-Parks estimates, it is consistent with the argument that congressional influence is most significant where centralized bureaucratic controls can operate, whereas local parties are more influential where local discretion is greatest.

Less supportive of the second hypothesis is the finding that Congress has a considerably greater impact than state representatives on penalty measures. Assuming that state representatives would spend more time in their (smaller) districts, they should presumably have more of a leadership impact than Congress on penalty activities. Table 1 indicates that this is not the case. One might argue that at least some congressional influence on penalties works through national headquarters rather than through local activities. However, given the assumption that penalties are difficult to control through the national office, the magnitude of congressional effects on penalty measures could still only be accounted for by attributing a greater influence to local activities by members of Congress (or their electoral coalitions) than to local activities by members of the state senate and assembly.

In sum, we cannot conclude whether local leadership by representatives or the activities of their supporting coalitions are more important in influencing local enforcement actions. The best case for the leadership argument is that local party leaders exert consistent influence on enforcement activities with greater local discretion, while members of Congress primarily rely on their influence in the national office. But this case has difficulty explaining the magnitude of congressional influence over the number and amount of penalties. The case for the supporting coalitions could plausibly argue that the local party and congressional coalitions are simply more important and influential over local bureaucratic activities than the electoral coalitions for the state senate and assembly. Perhaps the best conclusion at this stage is that the actions of both representatives and their electoral coalitions influence local enforcement, with some of the influence from members of Congress channeled through activities at the national office.

Hypothesis 4: The Influence of Political Culture

Field offices do not appear to be responding to a one-dimensional "political climate" rather than to distinct levels of representation and the corresponding electoral districts. A single index that sums the standardized congressional, state, and county-level measures is only marginally significant ($p < .1$) when it is the only political variable in the model. When the single index is included in the full model, Congress and local party variables remain significant, but the single index does not. This suggests that the individual measures have greater explanatory power than a single indicator of the county's political climate.

The poor performance of the single indicator is not surprising, given the weak relationship between the different political measures. The highest correlation between any pair of political variables is .5, and the amount of explained variance (R-squared) when each variable is regressed on the other political variables ranges from .25 for Congress to .43 for local party, with the two state variables falling in between. The differences in electoral district boundaries combined with the different sets of issues between candidates at different electoral levels apparently produces considerable diversity of representation.

In summary, the influence of the Reagan administration, congressional representatives, and local parties on county-level enforcement is quite strong, robust, and predictably reliable across different subsamples of the data. Their significance is little affected by differences in modeling or in data from different industries. The evidence supporting the influence of state senate and assembly officials is less clear; and the magnitude of influence is in any case much smaller than for the other political variables (with the exception of the impact of state assembly on inspections). The evidence also suggests that congressional influence is stronger in circumstances more favorable to central bureaucratic control over field agents, while local party influence is stronger in circumstances favoring the discretion of field agents.

Conclusion

We argue that field offices of federal bureaucracies respond to local electoral politics in a way that reinforces democratic controls. Our evidence confirms that county-level enforcement activities of one federal agency, OSHA, does indeed respond systematically to partisan differences in representatives elected to office at the local, state, and federal level. Given the stringent procedural requirements imposed on regulatory enforcement agencies like OSHA, we suspect that agencies less bound by such procedures exhibit even greater responsiveness to local partisanship than we found for OSHA.

Our analysis extends, rather than contradicts, the many studies that have found congressional and presidential influence over federal enforcement activities. We suggest that local influences are constrained by the federalist context and do not lead to a chaotic mosaic of feudal field offices. For example, the dummy variable representing the Reagan administration accounts for more county-level variance than any of the other variables. We note also that some of the influence of members of Congress appears to work through OSHA's national office, although even this influence works through individual members of Congress concerned with enforcement in their own electoral districts, not through the institutional influences of Congress and its committees on national enforcement policies.

How do local influences operate? We found little evidence of direct partisan intervention in particular cases under investigation, which might account for such influences in more service-oriented enforcement agencies. Consequently, the influence appears to be more indirect. The inevitable discretion of street-level bureaucrats occurs even in enforcement agencies, despite the volumes of regulations that govern their behavior and sophisticated management information systems that monitor their activities. Local partisan activities can provide enforcement resources and legitimacy that influence discretionary decisions.

We suggested three different explanations that could account for local partisan influence: leadership activities of representatives in their home districts, activities of the electoral coalitions, and the local political culture. The evidence did not support the explanation that political culture independently influenced both elections and bureaucratic behavior. We were less successful in determining whether it was the local activities of the representatives themselves or of groups in the electoral coalitions that influenced local enforcement activities. We suspect that representatives differ considerably in their local leadership role and therefore that influence shifts between the representative and the electoral coalition as one moves from county to county. The analysis did indicate clearly that the local and congressional electoral arenas are considerably more important than the state electoral arenas in their influence on OSHA.

Is local partisan influence on federal agencies a good thing? We noted that partisan conflict on the national level weakened both the incentives and ability of the national office fully to utilize techniques for controlling field office discretion. It is possible that OSHA's particularly contentious political environment may exaggerate responsiveness to local partisan influences, since minimizing the dissatisfaction of congressional representatives in their own districts may allow the national office to remove some of the steam from disagreements in Congress about appropriate levels of enforcement activities. Particularly for agencies in such contentious political environments, responsiveness to local partisan environments restores some of the democratic controls forfeited by the failure of central institutions to provide a stable democratic consensus as to what the agency is supposed to do.

Regardless of the turbulence of central political environments, local controls have the advantage of reflecting multiple compromises more sensitive to differences in local preferences than could be achieved in a single compromise at the national level. On the other hand, local controls also raise traditional democratic concerns associated with smaller political arenas more subject to dominance by locally powerful interests (Schattschneider 1960; Selznick 1966). While this concern is important, we emphasize that the partisan influence over bureaucracy that we have studied is directly related to elections; in other words, electoral institutions confront comparable problems in responding to local influences. Partisan influence is quite distinct from the commonly recognized form of local interest group influence on bureaucracy that is not subject to the challenges of forming electoral coalitions.

Local political influence also raises administrative concerns about the inefficiency introduced by political meddling when agencies are too open to outside influences (Seidman 1980). Particularly in the United States—where traditional distrust of bureaucratic authority, fragmentation of power, and adversarial and legalistic modes of decision making have weakened the ability of regulatory agencies to function as effectively as their counterparts in other western democracies (Kagan 1990)—the potential negative impact of local influence needs to be assessed carefully. On the other hand, recognition of local partisan controls as a legitimate source of influence in our federalist system may reduce the demand for more disruptive mechanisms to control bureaucratic decisions (Scholz 1989).

These normative issues cannot be resolved without greater knowledge about the operation and extent of local partisan controls. Understanding this broader form of influence by representatives and their electoral coalitions may become increasingly important in an era in which political initiative appears to be passing from federal to local political arenas, particularly since federal agencies spawned during the era of federal initiatives are likely to remain important players in local arenas.

Notes

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1. Partisanship = (party affiliation + [COPE Score/100]) − 1. The zero value for the midpoint is necessary because of the adjustment for district size discussed later.

2. The proportion was calculated separately for the period before and after the reapportionment affecting 1982 and later elections. The calculations were made using the number of registered voters in 1976 for the first period and 1982 for the second.

3. The function is based on the measure of full-time equivalent (FTE) representatives in the county, which ranged between .1 and 5.5 for congressional representatives, .2 and 8.6 for state senators, and .4 and 21.4 for state assembly representatives. We experimented with different power functions to find the value to which this measure needed to be raised in order to minimize the heteroscedasticity of the product of this correction function and the county partisan index. Raising the number of FTE representatives in a county to the 3.75 power minimized heteroscedasticity of the product for the state assembly. For consistency, the state senate and congressional measures also raised the corresponding FTE measure to the same power. For comparability between levels, the score for each legislature was divided by the mean of that legislature's corrective score across all counties. In short, the final measure for a given legislature (state assembly or state senate and Congress) represents the county's FTE measure to the 3.75 power multiplied by the county's partisan score (and divided by the overall mean corrective score for the legislative office).

4. The actual method of estimation subtracts the county mean of each variable from each observation, which eliminates the cumbersome need to add

a dummy variable to represent each county.

5. Since the dependent variable was the log of the enforcement measure, the magnitude of impact was calculated by multiplying the coefficient (.1) by the standard deviation of the logged variable, adding this to the mean of the logged variable, and calculating the antilog. The percentage change is the difference between this value and the mean divided by the mean.

6. The less robust influence in the state variables may be due to the difference between COPE scores used for Congress and NYCLU scores used for state measures. The COPE scores may tap the economic dimensions of ideology relevant to OSHA enforcement more accurately.

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BUSINESS POLITICAL POWER: THE CASE OF TAXATION

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We examine contending views about the forms and mechanisms of business power in U.S. politics by estimating time series models explaining taxation and redistribution. Taxation and redistribution constitute strong cases for theories about business and class power, since all firms have an interest in reducing taxation. We find that changes in corporate taxation and in redistribution between capital gains income and earned income and between corporate taxation and individual taxation are strongly influenced by political partisanship, with Democratic administrations increasing the tax burden on firms and their owners. How far corporations engage in electoral financing—measured through the establishment of corporate political action committees—is also influential. The models show some evidence consistent with the understanding of class power conceptualized by Bowles, Gordon, and Weisskopf and the presence of election cycle effects but inconsistent with implications arising from the structural dependence of the state on capital and asset concentration in the largest corporations as a mechanism of class power.

Few social science controversies are longer-lived than whether business firms and their owners dominate politics in democratic capitalist countries (e.g., Block 1977; Lindblom 1977) or not (e.g., Bauer, Pool, and Dexter 1963; Epstein 1969; Weidenbaum 1986; J. Wilson 1980). (See Epstein 1984; Jacobs 1985; and Windsor 1989 for reviews.) The debate is not resolved (nor will it be soon), in large measure because the issue of business power is as much ideological as scientific.

Failing at a conclusive *yes* or *no*, social scientists began recently to address the circumstances or contingencies under which the business firm and its owners exercise influence and power within particular issue areas. This second research stream remains closely related to the first since many of the contingency theorists' hypotheses are derived from the broader debate about business dominance (see Bowles, Gordon, and Weisskopf 1989; Jacobs 1988; Przeworski and Wallerstein 1988; Quinn 1988b; Swank 1989; Useem 1984; Vogel 1989).

We shall both extend recent contingency research and test an important set of general theses concerning business dominance. We take as our starting point a number of views about the relationship between the firm and the state. These include the thesis of the structural dependence of the state on capital, the class organization–class power model, and various partisan–electoral cycle theses. These perspectives yield distinctive, contending hypotheses about the mechanisms and forms of business power. The hypotheses are tested in time series models explaining corporate taxation and the redistribution of tax burdens. Taxation and redistribution are strong tests of theories of business power, since this is

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one area where all firms have a common interest, namely, to cut or shift their individual tax burdens.

Business Political Power

In his well-known review of Bauer, Pool, and Dexter's *American Business and Public Policy* (1963), Lowi argued that

> a political relationship is determined by the type of policy at stake, so that for every type of policy there is likely to be a distinctive type of political relationship. If power is defined as a share in the making of policy, or authoritative allocations, then the political relationship in question is a power relationship or, over time, a power structure. . . . There are three major categories of public policies in the scheme: distribution, regulation, and redistribution. . . . Each tends to develop its own characteristic political structure, political process, elites, and group relations. (1964, 688–89).

According to Lowi, business influence ought to vary from *strong* (like other interests) in distributive policies to *challenged* (and sometimes defeated) in regulatory matters to *generally decisive* in redistributive issues (see also J. Wilson 1980).

Lindblom (1984) extended Lowi's analysis by dividing issues into those that are settled within "small circles," those that command "public attention," and those that never make it onto the public agenda. Business influence on politics should vary by issue, being least powerful in arenas that command public attention (see also Bachrach and Baratz 1962; Schattschneider 1960). For both Lowi and Lindblom, an issue such as national environmental regulation (Vogel 1986) is highly public, and business influence will be relatively weak. Lindblom argues that we cannot generalize about business power in other policy areas from evidence taken from national regulatory policy making.

Lindblom, Block, and others think business influence ought to be decisive on policies directed toward investment and economic growth. Investment and growth policies ought, therefore, to be an ideal case for testing implications of the thesis of business dominance.[1] If we find contradictory evidence, the general business dominance thesis would be hard to sustain.

Analyzing the determinants of business taxation allows us to examine investment and economic growth policies in the context of redistribution. No firm seeks to pay higher taxes, since tax rates strongly influence firm profits. Firms, however, are not the only actors with a stake in business taxation. Business taxes are generally inversely related to wage taxation, so that reductions in one tend to be offset by increases in the other. Changing business tax rates strongly influence future firm investment, which in turn strongly affects the rate of economic growth (Boskin 1988). Further, victorious political parties use business taxation rates, in conjunction with interest rate policies, to implement economic growth strategies (Quinn and Shapiro 1991; on taxation and business power, see Bowles, Gordon, and Weisskopf 1989; Jacobs 1988; Martin 1991; Quinn 1988b; Salamon and Siegfried 1977; Swank 1989; Vogel 1989).

Four persuasive perspectives have been offered for analyzing firm and owner influence. Three specify which forms and mechanisms of business influence matter: the structural-dependence-of-the-state-on-capital model, partisan–electoral views, and the class organization–class power model. The fourth locates the discussion of business power in the U.S. institutional context.

The Structural Dependence of the State on Capital

The structural dependence thesis is the most influential perspective on business power (Vogel 1987). Versions of structural dependence have been advanced by Block (1977), Lindblom (1977), and Offe (1984). The thesis contends that no overt mechanisms of influence are necessary for the firm and its owners to exert power in capitalist democracies; its adherents reject "the idea of a conscious, politically directive ruling class" (Block 1987, 67). All that is necessary is that voters hold politicians accountable for the performance of the economy and that politicians be dependent on the voluntary market transactions of business firms to provide investment, employment, and economic growth. This incentive structure leads political leaders to provide inducements and other benefits to the firm. The privileges of business firms are not challenged, because restrictive government policies decrease investment, employment, government receipts, and economic growth. Voter unhappiness follows. "Although businessmen do not get everything they want," Lindblom writes, "they get a great deal. And when they do not get enough, recession or stagnation is a consequence" (1977, 187). Block concurs that "a sharp decline in business confidence leads to a parallel economic downturn" (1987, 61). For these analysts, the key mechanism of business influence on taxation and redistribution is the state's structural dependence on firms and their owners for investment.

Political Parties and Electoral Politics

The recent (post-1985) quantitative political economy literature has shown strong support for the partisanship thesis, which is that Left and Right governments are associated with differing macroeconomic characteristics, such as employment levels, monetary growth rates, income distributions, growth strategies, and growth rates. At the root of the partisanship thesis is the idea that parties draw on different social bases of support and that the power of contending groups will fluctuate with the fortunes of their allied parties. In general, these findings suggest that business power is greater under administrations of the Right (see Alesina 1988; Alt 1985; Chappell and Keech 1988; Hibbs 1987; Hicks 1988; Lange and Garrett 1985, 1987; see also Jackman 1987; Quinn and Shapiro 1991; Woolley 1988).

An additional political effect may be that of election cycles (Tufte 1978), in which a party appeals to supporters of its opponents during an election. For instance, Republican administrations might appeal during an election to the constituency of the Democratic party through tax policies favoring employee over business interests.

Class Organizational and Resource Factors

The resources that business organizations can bring to bear on the political process are a determinant of business power. But what are the mechanisms that facilitate the application of business resources to the political process, and what are some of the organizational preconditions for mobilizing and coordinating business resources?

Business influence, as with that of many other interests, is manifested through both electoral and governmental activities, especially political action committee (PAC) and lobbying activities. Considerable research effort has gone into understanding the circumstances that give rise to various forms of corporate political activity, especially the creation of business PACs, the behavior of these PACs, and the lobbying efforts of business firms (see Boies 1989; Burris 1987; Clawson, Neustadtl, and Beardon 1986; Masters and Keim 1985; Neustadtl and Clawson 1988; and Schlozman and Tierney 1986).

Which forms of organization might matter for influencing tax and investment policy is uncertain, however. For example, Edsall (1988) ties various changes in federal tax policy to the behavior of corporate PACs. In contrast, Biersack and Wilcox, in reviewing the literature, note that "the popular presumption that PACs buy votes finds little support in political science research, which has generally concluded that constituent attitudes, party, and ideology are much more important determinants of legislative votes than are PAC contributions" (1990, 231). Lobbying, in turn, has been seen as both a reaction to changes in public policy (Epstein 1984) and as a crucial vehicle for business influence (Fox 1982). Umbrella organizations such as the National Association of Manufacturers, however, are not regarded by corporate leaders as being an effective vehicle for influencing policy (G. Wilson 1990).

Other aspects of class (i.e., capitalist) organization might allow businesses to mobilize political resources. Interlinking corporate directorates, shared social ties, and shared ideology provide a basis for joint corporate activity and for overcoming collective action problems (for reviews, see Schwartz 1987; Useem 1984). In a study of capital taxation built upon this insight, Jacobs (1988) observed that for reasons described by Olson (1965) the degree of concentration of the country's asset base in the largest firms ought to influence the corporate tax rate. Simply put, Jacobs's thesis is that when the asset concentration ratio (percentage of total assets held by the hundred largest firms) increases from 45% to 45.5% (as his measure did from 1975 to 1976), big firms will have a greater incentive to cooperate in working for tax decreases. If Jacobs were correct, asset concentration would be an indicator of class political organization and class power.

Bowles, Gordon, and Weisskopf (1989) agree that the resources available to the capitalist class affect its political power. Using a regression analysis of firm profits on various exogenous variables, Bowles, Gordon, and Weisskopf developed a measure of class power based on microeconomic, firm-specific conditions: product market tightness, labor costs, strikes, measures of government regulations, and technological innovation. For them, it is neither asset concentration nor elite cohesion but the firm-specific microeconomy that determines the power of the owners of firms.

For both Jacobs and Bowles, Gordon, and Weisskopf, which mechanisms firms use to influence the political process matter less than which aspects of corporate resource organization allow firms to mobilize resources. These two theories do not specify the same construct, however, since the measures of corporate class power are *negatively* associated with each other.[2]

Political Institutions

The question of business influence cannot be abstracted from the institutional context in which the contest for power takes place. Political institutions have both traditions and routines (which may be referred to as their "structures") and collective goals (which constitute their "interests" in a Weberian sense; see Krasner 1978, 1984; Quinn 1988a; Skowronek 1982). Accordingly, government policies and goals have momentum; the state is not necessarily a captive of the capitalist elite; and agencies have a significant degree of autonomy, competence, and professionalism (J. Wilson 1980).

On tax and other policies, the structure and interests of the state condition both the target and the force of business power. For example, because the government must pay its bills and corporate taxation remains an important source of budget receipts, entirely eliminating the corporation tax is an unreachable target, no matter how politically powerful business is. Moreover, previous political agreements over the levels of taxation and the distribution of the tax burden are now embedded in the tax code, and these restrict the domain of what firms may achieve: tax policy is not a zero-based policy. To take a further example, since economic growth is a central goal for the U.S. government and tax policy is used to promote investment and growth (Hall and Taylor 1988), changes in tax policy that are at odds with prevailing macroeconomic theories about growth are extremely unlikely (but see Martin 1991).

Government structures also partly determine the mechanisms of political participation that allow for the exercise of business power. Of concern for our study, corporate campaign and electoral contributions were illegal under 18 U.S.C. 610 (1951) until the passage of the 1971 and 1974 Federal Election Campaign Acts. Other forms of political activities by corporations were in the past regarded by corporations as being also of questionable legality (Epstein 1968). Consequently, as Epstein shows, the great majority of corporations engaged in limited or no overt involvement in national politics before the late 1960s (1969, 67–107). With the change in electoral financing rules during the early 1970s has come a change in the mechanism of business influence, one that has arguably transformed U.S. politics (see Edsall 1988, 112–128, 209–221); nearly half of the Fortune 500, however, did not sponsor PACs in 1980 (Boies 1989). In summary, institutions shape the mechanisms and forms of business power.

Hypotheses Concerning Business Taxation and Tax Redistribution

Structural Dependence. This thesis specifies an always present incentive structure as the central mechanism of business power, which makes it difficult to operationalize. Several implications follow from the argument, however, and these implications may be operationalized.

The first implication is that in periods of economic difficulty, the investment function will take precedence over other public policy goals. As better economic times return, business's needs will be challenged by the claims of other competing interests, and other public goals will be less subservient to investment and growth (Lindblom 1977, chap. 13; Quinn 1988b; Swank 1989). Under the structural view, we expect business taxation to respond positively to economic activity: growth in investment and gross domestic product (GDP). As investment or the rate of growth falls, governments will cut taxes or shift the tax burden away from the firm and its owners so as to stimulate investment and growth. Therefore, the structural thesis holds that decreasing investment or decreasing economic growth brings forth policies designed to induce the firm and its owners to invest. We ought to find a *positive* association between investment and business taxation and between GNP and business taxation—that is, lower rates of investment will cause governments to lower the tax rates.

Second, the structural thesis claims that neither political party nor the political organization of firms and owners ought to influence tax rates. If we find political effects, these will be perverse—with parties undertaking policies at odds with either the presumed class interests of their supporters or the party's electoral interests. This will occur when the anticipation of government policies disadvantageous to business precipitates economic crises (e.g., investment strikes) that then lead governments to undertake counteracting policies at odds with their original intentions. (On the dynamics of structural constraints and anticipated policies, see Przeworski and Wallerstein 1988, 22–23.)

Political Partisanship. If the partisanship thesis is correct, business taxation should vary with political party, with Democrats increasing taxation and Republicans decreasing it (Hicks 1986; Quinn and Shapiro 1991). Democratic administrations should also seek to shift the tax burden to the firm and its owners. An alternative hypothesis is that parties do not differ, especially regarding growth and investment policies (Jackman 1986). Instead, we expect to find both parties seeking to decrease taxation during an election year and both seeking a postelection reequilibriating tax increase.

Class Organization and Class Power. According to this model, business taxation will vary with the organization of business resources or the organization of the business elite. Four measures of class organization–class power are offered: the election financing organization of business firms, the governmental lobbying organization of business firms, the assets controlled by the largest firms, and the microeconomically based index of class power developed by Bowles, Gordon, and Weisskopf.

Political Institutions. Previous policy choices and past political bargains will partly determine current tax policy, so that current tax levels will depend on past tax levels; and the same will be true for tax redistribution levels. (Employing the past levels of taxation also allows us to control for the presence of important but unobservable influences on policy; see Jacobson 1990.)

Our analysis of state institutions also leads us to expect a strong *negative* association between taxation and investment, in contrast to the structural dependence thesis. The negative association arises from the investment-promoting properties of the tax code.

The logic of government stimulation of firm investment is straightforward. Most investments are funded by a corporation internally from profits. With an investment tax credit (ITC), an investment is subsidized by the IRS, so that an increase in investment under an ITC will *reduce* the taxes paid during the *current* period both absolutely and as a percentage of pretax income: $+I_t \rightarrow -T_t$. With accelerated depreciation schedules, a greater part of the cost of an investment may be more quickly turned into business expenses than under traditional depreciation methods (e.g., straight-line depreciation). This also *reduces* taxes paid in the current and subsequent periods both absolutely and as a percentage of pretax income: $+I_t \rightarrow -T_t, T_{t+n}, \ldots$ The tax deductibility of interest payments also provides an implicit investment subsidy. With these provisions, the U.S. Tax Code contains investment incentives. If a negative relationship is not found, it would suggest that tax policy is ineffective.

A modified view of structural dependence is compatible with a negative association between investment and taxation *if certain conditions hold*, however. The simple version of structural dependence posits (correctly) that inducing firms to increase investment reduces short- and medium-term firm profits. Hence, a negative association between investment and taxation is at odds with structural dependence. But a negative association between taxation and investment might indicate the means through which government (in Block's language, "state managers") responds to business's structural power *if* lower business taxes induced higher investment, which then brought increased budget receipts to government: supply-side economics. The tax code forces businesses to shift income from profits to investment. The resulting economic gains will then produce more resources for the state from all forms of tax collection than would the initial collection of taxes from corporations. A negative association between investment and business taxation *in conjunction with* a positive association

Table 1. Hypotheses Regarding Business Taxation and Tax Burden Redistribution

Independent Variables	Changes in Tax	Changes in Redistribution	Tax Receipts
Structural views			
Democrats	$\beta = 0$	$\beta = 0$	
PACs	$\beta = 0$	$\beta = 0$	
Lobbyists	$\beta = 0$	$\beta = 0$	
Assets	$\beta = 0$	$\beta = 0$	
Bowles-Gordon-Weisskopf	$\beta = 0$	$\beta = 0$	
Gross domestic product	$\beta > 0$	$\beta > 0$	
Investment$_1$	$\beta > 0$	$\beta > 0$	
or			
Investment$_2$	$\beta < 0$	$\beta < 0$	$\beta > 0$
Partisanship			
Democrats	$\beta > 0$	$\beta > 0$	
Election cycle			
Election years	$\beta < 0$	$\beta < 0$	
Postelection (both parties)	$\beta > 0$	$\beta > 0$	
Class organization-class power			
PACs	$\beta < 0$	$\beta < 0$	
Lobbyists	$\beta < 0$	$\beta < 0$	
Assets	$\beta < 0$	$\beta < 0$	
Bowles-Gordon-Weisskopf	$\beta < 0$	$\beta < 0$	
Institutional control variables			
Lagged dependent	$\beta > 0$	$\beta > 0$	
Investment	$\beta < 0$	$\beta < 0$	$\beta = 0$

Note: Business taxation = IRS measure of corporate tax rate = taxes paid as percentage of capital stock; redistribution = corporate taxes as percentage of individual taxes; government income = budget receipts as percentage of GDP.

between investment and overall tax receipts suggests structural power, though of a more complex sort. A negative effect of investment on taxation *absent* a positive association between investment and tax receipts would be inconsistent with structural dependence (see Investment$_1$ in Table 1).

The hypotheses are summarized in Table 1.

Models: Variables, Data, and Methods

Dependent Variables

We have two sets of tax models, one for corporate taxation and one for the redistribution of the tax burden. By *corporation* or *firm,* we mean nonfinancial corporations, both service and manufacturing. We have deliberately chosen to use the word *business* and not *capitalist* because a corporation is a legal entity, and taxation is assessed on it.

Our first corporate taxation measure is federal taxes paid by corporations divided by pretax profits as calculated by the IRS, and it comes from the Federal Reserve Bank's *Flow of Funds* (1989b) table for nonfinancial corporate business.[3] This measure is the price set by the federal government on corporate profits. It captures the effects of depreciation schedules and investment tax credits, which are reflected in exclusions from tax payments. Most importantly for testing the theories at hand, the IRS measure is the measure that the government alters when it seeks to redirect corporate investment. One disadvantage of this measure is that a change in the definition of taxable income intended to reduce corporate taxation might not be reflected in this ratio measure, since the value of both the numerator and the denominator would decrease.

We also report results for a second measure, namely, corporate taxes paid divided by the total capital stock, which is reported in the Federal Reserve Bank's *Balance Sheets* (1989a). This tax on assets measure has the same numerator as our first measure, but the denominator is the nation's nonfinancial corporate capital base. Because the denominator is unaffected by tax policy changes, the measure gives us information about the inflation-adjusted level of tax collections. Many economists believe that this measure is a more representative measure of the real tax burden of corporations than the IRS measure (see the discussion in Fullerton 1984). We will have strong confidence in our results only if similar effects are found for both measures.

An alternative to our second measure is to use one of the 30 or so competing measures of corporate or capital taxation proposed by various economists and accountants: tax payments as a percentage of some variation of a computed measure of investment, equity, or assets. (Twenty-six effective tax rate measures are reviewed and compared in Fullerton 1984.) These competing estimates vary widely, however, for reasons obscure to the noncorporate finance economist. For example, Feldstein and Summers (1979) compute the average effective corporate tax rate as being 66%, while King and Fullerton (1984) say 37%.

Obviously, econometric results will differ if estimates of the dependent variable differ. We analyzed four identical models of taxation for 1954–79 but used differing computations of taxation by well-known economists: Holland and Myers (1980: used by Jacobs 1988), Pechman (1987), Fama (1981), and Hulten and Robertson (1984). We found widely varying coefficient estimates and differing signs for many independent variables. For example, Jacobs's findings regarding asset concentration were replicated only when using the Holland and Myers measure. These varying coefficient estimates and signs point to the need to have strong theoretical reasons both for choosing not to use the tax data provided by the IRS and Federal Reserve and for choosing one of these competing computed measures of taxation over others.

The redistribution equations also use actual tax payments as dependent variables. In the redistributional model, the total taxes paid by corporations are divided by the total tax paid by individuals.[4] The individual wage and corporate tax data are taken from the *Budget of the United States, 1989* (historical tables).

In order to test one implication of structural dependence, we specify another model with the federal government's total budget receipts as a percentage of gross national product as the dependent variable. The data are taken from the *Budget of the United States, 1990* (historical tables).

Independent Variables

Party is operationalized as a dummy variable for the party of the president (1 = Democratic, 0 = Republican), which is appropriate, since the presidency is the main political influence on tax policy. Tax bills generally originate from the executive, and only one major tax bill (a 1980 oil import fee imposed by President Carter) was passed over presidential veto during the period we analyze (Pechman 1987). Analysis including dummy variables to control for party dominance of the Senate (the only party change in Congress during the period) showed no significant effects and are excluded.

The effects of business PACs are operationalized as the proportion of PACs that are corporate-sponsored. The assumption behind this operationalization is that the greater the number of business PACs, the greater the incentive for politicians to reduce the corporate tax burden through firm-or-industry-specific exemptions and credits, as well as through more general reductions in the tax rates.

The PAC measure is taken from Federal Election Commission data, which are reported in Stanley and Niemi (1990). The data begin in 1974 with the establishment of the Federal Election Commission. The years prior to 1974 are scored with zeros; and this scoring reflects the legal changes of the early 1970s, which legalized corporate-sponsored electoral activities. We know, of course, from the Watergate affair that corporations did make illegal campaign contributions prior to 1974, but we have no way of operationalizing what was illegal.

We did not operationalize business PAC effects through corporate campaign contributions for two reasons. First, the spending by type of PAC (e.g., corporate, labor) is first reported systematically by the Federal Election Commission for the 1977–78 election cycle.[5] Second, the contribution data are generally reported in two-year cycles (i.e., 1977–78, 1979–80, etc.). In order to estimate our equations, stable annual data with many data points are necessary, and this measure serves that purpose better than any other. The measure, when averaged over a two-year campaign cycle, is correlated at .89 with corporate campaign contribution data.[6]

Business governmental activities are operationalized as the annual percentage of registered lobbyists for all business organizations. The 1946 Federal Regulation of Lobbying Act requires those organizations and individuals who seek to influence Congress directly to register with either the clerk of the House or the secretary of the Senate. Our measure is computed from these registrations, which are compiled and reported in the *Congressional Quarterly Weekly* and the *Annual Almanac.*[7]

We estimated previously the effects of the Democratic and Republican party pre- and postelection year dummy variables (Quinn and Shapiro 1991). The only significant effects were found for Republican first years in office and for election year "runs to the middle." These are included in the models where appropriate.

In representing the macroeconomy, we follow the basic structure of the standard demand-driven, IS–LM model (Brooks 1984). In these models, taxes are determined by the growth of real output, which we represent using the growth of gross domestic product, growth in the plant and equipment capital stock, and changes in the current accounts balance as a percentage of GDP. Even though the demand-driven, IS–LM model has been challenged by adherents of other macroeconometric schools (e.g., rational expectations), it remains the underpinning of the great majority of the macroeconometric models (Brooks 1984; see Fair 1990 for a discussion of macroeconometric models). In line with the assumptions of the IS–LM model, our analysis assumes that expectations of future tax policy changes do not determine current economic states.

The rate of growth in real gross domestic product is taken from the Organization for Economic Cooperation and Development's *Historical Statistics* (1989), except for the data from 1954–64, which are found in the U.S. Bureau of the Census' *Historical Statistics of the United States* (1975). The plant and equipment investment figure is taken from the Federal Reserve Bank's *Flow of Funds* (1989b) and is deflated by dividing into it the plant and equipment capital stock, reported in the Federal Reserve Bank's *Balance Sheets* (1989a). The plant and equipment figure is, then, the yearly increase in the plant and equipment stock of the nation's nonfinancial corporate enterprises.

We have not only macroeconomic reasons for including some measure of the trade deficit in our model. Several recent works have emphasized the role of the international economy in shaping domestic political institutions and policies (Cameron 1978; Lange and Garrett 1987; Katzenstein 1985). To account for international effects, such as the oil shock, the current account balance as a percentage of gross domestic product is taken from the International Monetary Fund's *Statistics Yearbook* (1989). Current accounts measures net exports, and include the effects of oil imports. This measure is widely used in macroeconometric modeling to operationalize international effects (Brooks 1984).

The asset concentration measure is the year-end assets of the Fortune 100 industrials divided by the total nonfinancial corporate assets from the *Balance Sheets.* This measure is positively statistically related—but not identical—to the measure reported in the *Statistical Abstract* and used by Jacobs (1988).[8] Both asset concentration measures, however, might be associated with taxation for reasons having nothing to do with capitalist class organization if scope and scale affected investment, which then affected asset concentration (see Quinn 1989; Jacobs 1989; see Appendix).

A potential problem in estimating the equations is that the Bowles, Gordon, and Weisskopf measure of class power contains information that is also contained in two of our other independent variables. Their *government regulation* might be related to our Democratic party dummy variable, and their *import penetration* might be related to current account balances. We interpret this to mean that part of the effect of their measure of power represents the effects of the other independent variables. (Their index, stripped of its tax component, was provided by David Gordon.)

In all equations, we use a lagged endogenous variable, $Y_{(t-1)}$, because this year's levels of taxation, tax distribution, and budget receipts are each partly dependent on previous states. Serial correlation was present (as measured by critical values of Durbin's h and by further specification tests of our coefficients)[9] in one of our initial ordinary least squares regression models of corporate taxation. Here we use an instrumental variable method for estimating time series equations with a lagged endogenous variable (see Ostrom 1978).[10]

The effects of most independent variables (except the lagged endogenous one) are estimated contemporaneously with the dependent variables, since economic and political conditions at time t determine the value of tax rates at time t, while the tax rates most directly affect the economy at time $t + n$. For example, the 1989 corporate taxation bill will be calculated in 1990, using 1989 data. These 1989 financial results will be determined by previous states (reflected in the lagged endogenous variable) and by the events of 1989. The 1989 tax bill, calculated and paid in 1990, will not affect the independent variables from 1989.

Three other variables are lagged. The asset concentration is lagged by Jacobs (1988), and we follow his practice. The business lobby measure is lagged because the lobbyist registrations follow the congressional calendar: 1963 data include data from October 1962 through August 1963. Gross domestic product is lagged one year, owing to the possibility that corporate tax prepayments might affect the current growth rate. We also lag the investment variable in the tax-as-a-percentage-of-assets model, since the denominator of the tax (dependent) variable is closely related to the denominator of the investment (independent) variable.

The investment data are only available annually. We use data from 1954 on, for two reasons. First, the Internal Revenue

Code of 1954 was the first modern corporate tax act: it introduced accelerated depreciation. This constitutes the major "regime shift" in tax policy during this period, and we have reason to be cautious about the parameter estimates of tax equations that include data on both sides of 1954 (see Fair 1990; Lucas 1976). Second, a 1954 Supreme Court case finally settled the question of which lobbyists must register under the 1946 Federal Regulation of Lobbying Act, so that the lobbying data prior to 1954 are incomparable to the data thereafter.

Results

We begin with equations for corporate taxation. Table 2 contains five specifications for the IRS tax rate. The estimates are robust across equations, in which variables are added and excluded. All five models perform extremely well, with no evidence of serial correlation.

Each model has a strong and positive coefficient for Democratic party, as is predicted by the partisan view of politics. The value of the coefficient corresponds to a 1.5%-2.5% increase in mean busi-

Table 2. Effects on IRS Corporate Taxation Rates, 1955–1987: Business Taxation, Taxes Paid/Pretax Profits

Independent Variables	Models^a 1	2	3	4	5
Lagged dependent	.419 (2.84)**	.546 (3.42)**	.534 (3.44)**	.513 (3.49)**	.530 (3.48)**
Growth rate$_{t-1}$	.055 (.46)	.063 (.52)	.095 (.81)	.014 (1.17)	.014 (1.10)
Plant and equipment investment	-1.23 (-3.91)**	-1.10 (3.63)**	-1.31 (-4.06)**	-1.53 (-4.54)**	-1.53 (-4.38)**
Current accounts balance	-1.19 (-2.80)**	-1.17 (-2.98)**	-1.11 (-2.79)**	-1.13 (-3.13)**	-1.17 (-3.38)**
Party—Democratic administration	1.56 (1.74)	1.88 (2.11)*	2.34 (2.53)**	2.45 (2.87)**	2.32 (2.61)**
Election cycle Republican first year	1.34 (1.71)	1.55 (1.92)	1.48 (1.92)	1.83 (2.44)**	1.99 (2.41)**
Number of corporate PACs as % of total	-.138 (-3.45)**	-.096 (-2.11)*	-.136 (-2.65)**	-.106 (-2.52)**	-.078 (-1.44)
Business lobbyists as % of total	— —	.089 (1.54)	.082 (1.49)	.080 (1.50)	.083 (1.50)
Bowles-Gordon-Weisskopf class power measures	— —	— —	-2.50 (-1.47)	— —	1.73 (.66)
Asset concentration Fortune 100$_{t-1}$	— —	— —	— —	.908 (2.25)**	1.22 (1.77)
Constant	42.0 (5.08)**	29.322 (2.66)**	40.1 (3.22)**	18.7 (1.68)	6.42 (.32)
DW^b	1.62	1.74	1.65	1.74	1.82
$\hat{R}^2$	.90	.91	.91	.92	.92

Note: t-ratios are in parentheses.

[a] Instrumental lagged endogenous variable, AR1.

[b] With instrumental lagged endogenous variable, DW becomes interpretable again.

$^*p < .05$, two-tailed test.
$^{**}p < .01$, two-tailed test.

ness taxation associated with a Democratic administration.

The Republican postelection dummy has a strong and positive coefficient, suggesting that first-year Republican administrations increase business taxation on average, the 1981 Economic Recovery Tax Act notwithstanding. It is, however, a smaller effect than that of the overall Democratic party dummy; and it is not replicated in the other tax models.

The investment coefficients are negative and strongly significant in all equations. The effect of the lagged endogenous variable is positive and significant in all equations. The coefficient for the rate of economic growth is insignificant, though the sign is consistent with expectations. The current accounts balance coefficients are significant but negative—an apparent puzzle, though this finding may have resulted from the inclusion (after 1980) of the profits of subsidiaries of Japanese and other foreign companies in the taxation data.[11]

The business PAC coefficient is negative and significant in all but the final equation, where we suspect, from the correlation matrix, that multicollinearity is present. This means that the greater the number of business PACs relative to other PACs, the lower the taxes on business firms. The coefficient's size and sign indicate that the efforts by business firms in financing political campaigns have the expected effects.

The business lobbying coefficient, however, is not significant (though it is one-and-a-half times its standard error). The sign is positive, which is the opposite of expectations. Perhaps business lobbying is not effective or is reactive to adverse policies. Comparing the sign and significance of the PAC and lobbying coefficients leads us to the conclusion that electoral corporate involvement is an important mechanism of business political power.

The Bowles, Gordon, and Weisskopf measure is not significant in model 3, but it has the sign forecast for it by them; and it is one-and-a-half times its standard error. This suggests that as this microeconomic measure of class power decreases, the taxation of business firms increases. The measure, when included in a model with asset concentration, has a coefficient of roughly the same magnitude, but the sign reverses. Another point worth repeating is that the Bowles, Gordon, and Weisskopf measure includes information in the index of class power about the trade balance and, perhaps, about party; thus, multicollinearity (among party, trade, and the Bowles, Gordon, and Weisskopf class power measure) potentially diminishes its effects.[12]

The asset concentration measure in model 4 has a *positive* and significant coefficient. Our finding is opposite to Jacobs's (1988) but is consistent with that of Salamon and Siegfried (1977). As assets become more concentrated, corporate taxes don't decrease, they increase. The problem of multicollinearity also appears here, we suspect, for the estimation of asset concentration in the general model with all the variables.

In Table 3, the dependent variable is tax payments as a percentage of total corporate capital. The coefficients are stable and consistent across models. The Democratic party variable is always positive and significant. No election cycle variables approached significance, and they are excluded from the equations. The business PAC measure is negative and significant in three models, though multicollinearity appears again in models 4 and 5.[13]

Here, too, investment has a negative and significant effect in all models, and the coefficient for the lagged endogenous variable is strongly positive and significant. The growth rate and the current accounts balance do not have significant effects; neither do the Bowles, Gordon, and Weisskopf nor the asset concentration measures, though the signs in models 3 and 4 are consistent with the signs in the corresponding corporate taxation models in Table 2.

Table 4 shows the results of the budget receipts as a percentage of GDP model. Recall that a negative association between investment and taxation is consistent with structural dependence *if* a decrease in corporate taxation that resulted from investment incentives subsequently produced an increase in revenues from total budget receipts: a successful supply-side economics strategy. The test is to see whether state revenue receipts respond positively to various lags of investment.

Business investment is apparently not associated directly with increases in federal budget receipts in any of the three models in Table 4. The effects of investment might, however, be mediated over a longer period of time through increases in the growth rate.

The redistribution equations show striking effects of political partisanship, adding further support to the partisan thesis vis-à-vis redistribution (see Hibbs 1987; Hicks and Swank 1984; Swank 1989). Table 5's dependent variable is corporate tax receipts as a percentage of individual income receipts. All five models perform exceptionally well, and the

Table 3. Effects on Corporate Taxation Rates, 1955–1987: Taxation As a Percentage of Total Assets

	Models[a]				
Independent Variables	1	2	3	4	5
Lagged dependent	.774 (7.21)**	.772 (6.99)**	.837 (5.10)**	.823 (6.15)**	.806 (4.44)**
Growth rate$_{t-1}$	–.019 (–1.09)	–.020 (–1.07)	–.021 (–1.11)	–.020 (–1.04)	–.019 (–.93)
Plant and equipment investment$_{t-1}$	–.091 (–3.49)**	–.089 (–2.90)**	–.106 (–2.39)*	–.108 (–2.63)*	.105 (–2.31)*
Current accounts balance	–.060 (–1.36)	–.062 (–1.32)	–.064 (–1.36)	–.060 (–1.30)	–.059 (–1.20)
Party—Democratic administration	.241 (2.84)**	.239 (2.75)*	.254 (2.75)*	.237 (2.69)*	.227 (2.02)
Number of corporate PACs as % of total	–.008 (–2.64)*	–.008 (–2.18)*	–.008 (–2.15)*	–.007 (–1.56)	–.006 (–1.05)
Business lobbyists as % of total	— —	–.001 (–.15)	.001 (.127)	.001 (.084)	.000 (.01)
Bowles-Gordon-Weisskopf class power measure	— —	— —	–.118 (–.336)	— —	.066 (.14)
Asset concentration Fortune 100$_{t-1}$	— —	— —	— —	.030 (.695)	.041 (.44)
Constant	1.73 (4.06)**	1.78 (3.24)**	2.05 (2.77)**	1.19 (1.18)	.81 (.28)
Durbin's *h*	–.63n.s.	–.57n.s.	–1.63n.s.	.79n.s.	–.33n.s.
$\bar{R}^2$	.92	.92	.92	.92	.91

Note: t-ratios are in parentheses.

[a]Lagged endogenous variable, OLS.

$^*p < .05$, two-tailed test.
$^{**}p < .01$, two-tailed test.

Table 4. Investment and GDP Effects on Budget Receipts, 1955–1987: Budget Receipts As a Percentage of GDP

	Models[a]		
Independent Variables	1	2	3
Lagged dependent	.727 (5.85)**	.728 (5.75)**	.739 (5.62)**
Growth rate$_{t-1}$	.160 (3.70)**	.168 (2.43)*	.125 (1.61)
Plant and equipment investment$_{t-1}$	.054 (.68)	.038 (.27)	.157 (.94)
Plant and equipment investment$_{t-2}$	— —	.022 (.14)	-.185 (-.83)
Plant and equipment investment$_{t-3}$	— —	— —	.154 (1.30)
Constant	2.85 (1.50)	2.74 (1.31)	1.90 (.83)
Durbin's *h*	-.07n.s.	-.11n.s.	.52n.s.
$\bar{R}^2$	.58	.57	.56

Note: t-ratios are in parentheses.

[a] Lagged endogenous variable, OLS.

$^*p < .05$, two-tailed test.

$^{**}p < .01$, two-tailed test.

parameter estimates are stable across models.

The election cycle variables are now significant and are reported in the table. These variables show evidence of a "run to the middle" by both parties during an election year; that is, as an election approaches, parties apparently reverse normal policies, with Democratic administrations shifting the tax burden toward individuals and away from the corporation and the Republicans doing the reverse. This finding is consistent with arguments about the political election cycle. The coefficient of the Democratic party dummy variable is, once again, strongly positive each time.

We have no theoretical reason for believing that the economic variables affect the distribution of the tax burden between wage-earners and corporations. In fact, preliminary tests showed that none of the economic variables approached significance in the models, so they are not included.

Once more, the business PAC measure shows a persistent negative effect, though multicollinearity appears to inflate the standard errors of the PAC measure in models 2–5. The lobbying measure is once again positive, contra expectations (though insignificant). Neither the Bowles, Gordon, and Weisskopf measure nor the asset concentration measure has significant effects, though the asset concentration measure has for the first time the hypothesized negative sign. In this model, all the effects are political, and business power appears to be mediated through party and through electoral financing organizations.

We also tested a model using as the dependent variable the ratio of the tax receipts from individual capital gains to the tax receipts from individual earned income. The Democratic party dummy

Table 5. Tax Redistribution: Corporate Income Receipts As a Percentage of Earned Individual Income Receipts, 1954–1987

	Models[a]				
Independent Variables	1	2	3	4	5
Lagged endogenous variable	.772	.789	.763	.759	.810
	(8.15)**	(8.60)**	(7.25)**	(7.79)**	(7.34)**
Party—Democratic	2.04	2.19	2.06	2.12	2.60
	(2.70)*	(2.73)*	(2.43)*	(2.63)*	(2.76)*
Election year Democratic administration	-2.23	-2.30	2.19	-2.13	-2.23
	(-2.03)	(-2.06)*	(-1.90)	(-1.87)	(-1.95)
Election year Republican administration	2.63	2.78	2.64	2.59	2.81
	(3.08)**	(3.10)**	(2.78)*	(2.81)**	(2.95)**
Number of corporate PACs as % of total	-.047	-.036	-.032	-.042	-.076
	(-2.00)	(-1.24)	(-1.06)	(-1.40)	(-1.66)
Business lobbyists as % of total	—	.037	.032	.035	.059
	—	(.62)	(.524)	(.596)	(.91)
Bowles-Gordon-Weisskopf class power measure	—	—	.62	—	-3.21
	—	—	(.52)	—	(-.99)
Asset concentration Fortune 100_{t-1}	—	—	—	-.247	-.915
	—	—	—	(-.94)	(-1.26)
Constant	4.43	-1.80	-.92	7.47	27.3
	(2.01)	(-.38)	(-.18)	(.97)	(1.27)
Durbin's *h*	-.01n.s.	-.12n.s.	-.14n.s.	-.28n.s.	-.44n.s.
$\hat{R}^2$	.92	.92	.92	.92	.92

Note: *t*-ratios are in parentheses.

[a] Lagged endogenous variable, OLS.

$^{*}p < .05$, two-tailed test.

$^{**}p < .01$, two-tailed test.

variable is always positive and strongly significant, which is consistent with the partisanship thesis. This suggests that Democratic administrations shift the tax burden toward recipients of unearned income and away from the wage-earners, whereas the Republican administrations do the reverse. (No election cycle variables had effects, and they are excluded.) The corporate PAC variable is always negative but is significant only in the model without other class organization variables, with which it is collinear. The signs are negative, as expected, revealing that business interests and political presence apparently work to cut taxes for the wealthy, even though corporate PACs have no direct fiduciary interest in the capital gains taxation of individuals. No other class organization variable had a significant effect.

Discussion

Our investigation examined several hypothesized relationships involving taxation, redistribution, politics, and business/class power. The partisanship thesis receives unambiguous support. These results add to the substantial evidence for the partisan view of politics. For each dependent variable, in every model, the effect of the Democratic party variable was strong. Who governs matters for changes in corporate taxation, for the redistribution of the tax burden between owners of capital and wage-earners, and for the redistribution of the tax burden between wage-earners and the business firm.

The political election cycle hypothesis fared less well, since elections had no systematic effect on both corporate taxation models. One effect of elections did appear in the distribution of the tax burden between firms and wage-earners, with Democratic and Republican administrations pursuing policies the reverse of their usual ones in an election year shift to the center.

The class organization–class power hypothesis was examined using four variables. We found one mechanism of business influence to be effective: its campaign financing organizations. As measured through its (usually statistically significant) PAC representation, business's electoral organization translated into lower taxes.

The establishment and rapid growth of corporate and trade association PACs could, however, be connected to another of our independent variables, political partisanship. The Republican presidential ascendency in presidential elections roughly coincides with the 1971 and 1974 election financing acts and the subsequent mobilization of business interests to challenge what many businessmen regarded as Democratic-sponsored coercive government (Ginsberg and Shefter 1990, chap. 4). Although presidential campaigns are publicly funded, the mobilization of business interests helped create a climate favorable for Republican presidential candidates. Perhaps some of the effects of business PACs are represented in the partisanship variable.

Another proposed mechanism of business influence—its corporate governmental activities (as analyzed through a lobbying measure)—was never statistically significant and usually bore the sign opposite to expectations. This suggests that lobbying activities are a far less effective mechanism of business influence on corporate tax policy than are PACs. (See, however, Wright 1990 on the effectiveness of lobbying in other policy areas.)

A third aspect of class organization—the concentration of resources in the largest corporations proposed by Jacobs (1988)—has either an effect opposite to the hypothesized effects or no effect.[14] The significance of the PAC variable is also relevant for assessing the asset concentration thesis. Jacobs's analysis assumes that the greater the concentration of resources in fewer corporate actors, the lower the impediments to collective corporate action and the lower the corporate tax rate. Our measure of business PAC influence assumes, in contrast, that the greater the number of corporate actors, the lower the tax rates. Our finding of strong effects from an increasing proportion of business PACs offers some support for Mizruchi's (1989) argument that similar but uncoordinated behavior by corporations may significantly influence policy.

A fourth class power measure, the microeconomically based measure devised by Bowles, Gordon, and Weisskopf (1989), showed no significant effects. It did, however, approach significance in Table 2, model 3 and appears to contain information from other variables; so the evidence is less conclusive on this measure than for other variables.

The structural dependence thesis is difficult to test as it specifies an always-present incentive structure as the key mechanism of business power. Our strategy is to examine implications that follow from the argument. Four pieces of evidence are inconsistent with implications of structural dependence.

First, political parties matter for determining tax and investment policy. Structural dependence holds that who governs is irrelevant for business power; yet Democrats and Republicans pursue differing tax and redistribution policies, with probusiness policies much in evidence during Republican administrations. The Democratic party, in contrast, has strong redistributive effects and strong tax increasing effects.

Second, structural dependence specifies mechanisms that "operate independently of any political consciousness on the part of the ruling class" (Block 1987, 67). In fact, three proposed mechanisms of class organization showed no effects on taxation and redistribution. The electoral financing organization of business, however, clearly influences tax policy. If class consciousness and class organization are "superfluous" for business power, why do business PACs influence tax policy?

Third, investment has a strongly negative effect on corporate taxation. Decreasing investment does not lead to decreasing taxation (as one version of the structural dependence hypothesis implies) but to increasing taxation: the tax code has been designed such that business can reduce its taxes by increasing investment. The strong negative association between investment and taxation occurs absent subsequent positive effects of investment on government budget receipts. Block argues that state managers are concerned with the resource base of the state (1987, 65–67); yet business investment does not appear (at least over a three-to-four-year period) to increase budget receipts independently. In summary, decreasing investment does not appear to call forth systematically decreasing business taxation. Increasing investment does not appear to lead directly to increases in the resources available to state managers.

Fourth, state institutions matter in determining the forms and mechanisms of business influence. Previous policies have momentum, as witnessed by the always-significant lagged dependent variable. The investment coefficient on the two measures of corporate taxation is always negative and significant, as is consistent with a public policy goal of using the tax code to promote investment. We might also note that electoral opportunism, in the form of electorally motivated policy bursts, appears to be limited to the redistribution of the tax burden and to have little to no effects on corporate tax policy.

Conclusion

Our evidence is that the power of firms and their owners is apparently mediated through both party and PACs. No evidence supports the claim that business power is derived from the political organization that results from asset concentration in the largest firms, from business lobbying, or from various microeconomic factors. This suggests that firm and owner power regarding taxation, investment, and economic growth are mediated through two of the "overt faces of power"; election financing and political partisanship.

Our findings challenge the structural dependence thesis insofar as the structural dependence thesis argues that class consciousness, mediated through political and class organizations, does not matter: both who governs and who finances the elections matter greatly for tax policy.

We have not, however, refuted the thesis of the structural dependence; nor is it easy to see how it could be refuted. A defender of structural dependence might plausibly argue that if the United States had no conservative party allied with business and if it had no allowance for firm financing of elections, business influence would then be mediated through structural dependence. It may be that the United States has "surplus mechanisms" of business power and the implications of structural dependence do not now demonstrate themselves because of the plethora of other avenues of business influence. Further, even if business interests act as though they believe that overt organization is necessary for business power in the United States, it might not be so. We have not demonstrated in this investigation that structural dependence exists, but neither have we demonstrated that it does *not* exist. Structural dependence might reveal itself if business interests were ever strongly threatened in the United States.

Searching for business structural power is much like using a water-witching stick to dowse a field for underground water. It may have made sense to search for the structural base of business power in the draught of business political activity in the 1960s and early 1970s. In fact, the thesis of structural dependence emerged during this period, when business firms were not able to fund election campaigns, had yet to mobilize to meet the challenges posed by citizen action groups (Pertschuck 1982), were mostly uninvolved in Washington politics (Epstein 1969), and when the two major U.S. parties accepted Keynesian growth strategies that stressed the redistribution of income (Pechman 1987). Given these facts, structural dependence fit the U.S. case.

But since the late 1970s, the political field has been flooded by business power. Maybe there is water underground, but it matters less when floodwater covers the field. We hardly need the water-witching stick of structural dependence to find business power in Reagan's United States.

A reformulated version of the thesis of the structural dependence of the state on capital is consistent with our findings. This reformulation would stress the two-way, *joint* dependence of the state and firms on each other. This analysis of joint dependence follows from our interpretation of the negative effects of investment on taxation.

Capital investment is subject to diminishing marginal returns, just like any other input (Jones 1975); but the point of diminishing returns may differ for society, the firm, and the firm's owners. That is, the managers of firms, the owners of firms, and governments each have differing interests regarding both the level and the time horizon of investment.

Regarding the level of investment, the level of new investment needed to produce the optimal profit for each firm is, in the aggregate, *less* than that needed to produce the optimal level of output for society (Bowles, Gordon, and Weisskopf 1989). This is so because each firm and its managers (whose jobs and perquisites follow from their positions in the firm) bear the specific risk associated with investment, especially in new technology. Society, in contrast, is able to diversify away the risk of new technology.

In turn, the socially optimal investment is *less* than the level preferred by the owners of firms. This is so because the limited liability provisions of corporation law puts owners in a position analogous to that of the holders of an option (John, Senbat, Sundaram 1990; see also Banerjee and Besley 1990).

The owners, as holders of an option, are risk seekers and will overinvest. The managers of firms, having much greater downside risk, are risk-averse and will underinvest. Society prefers investment that minimizes the risk of firm bankruptcy and default while inducing a continuously increasing, higher level of new investment. Thus, each party has different preferences regarding level of investment.

The tax code may solve this three-way agency problem by providing a continuous incentive for investment through an economywide corporation tax rate that is tied to technology-specific investment deductions (John, Senbat, and Sundaram 1990). The tax code may be seen as a "treaty" of sorts among the firm, its owners, and society, wherein the state pushes out the frontier of new investment (i.e., more investment than firms would have otherwise provided) while targeting socially optimal investment through specific tax inducements. Firms reduce their tax bills, and owners benefit from profits resulting from investment in new technology; but government continues to receive tax revenues, since changing the marginal tax rates through tax incentives on *new* investment induces new investment without cutting the state's revenue stream from *older* investment.

Tax incentives also appear to change the time horizons of businesses. Firms prefer immediate cash flows to distant cash flows. Thanks to accelerated depreciation provisions and the (now-restricted) investment tax credit, the tax code allowed larger cash flows to follow more quickly from investment. Given that U.S. firms generally invest less than do Japanese and German firms, the tax code induces future investment that firms would not otherwise undertake, thereby producing higher levels of investment for future firm growth and competitiveness. The tax code may then cause the time horizon of firms to match more closely the time horizon of society.

The evidence regarding the negative association between investment and taxation is consistent with such a mutual dependence story. In return for lower taxes, firms invest more than they otherwise would and are able to do so without otherwise decreasing dividend payments to shareholders. The state and society receive higher levels of investment than they otherwise would, increasing future growth.

A defender of structural dependence could reformulate our analysis in structural dependence's language of description. It may very well be, as Poulantzas, Block, and Offe argue, that the state is dependent on capital; but the managers of the state may be able to "rationalize" the system (i.e., reform it) against the wishes of business firms. Thus, the state might increase taxes on capital so as to promote investment even against the unanimous wishes of all business firms. Here, the state promotes the interests of the system of capitalism, though not the interests of firms and owners.

This version of structural dependence, however, is untestable, since it can explain *any* form of business-government relations. It is a nonfalsifiable language of description, not amenable to social-scientific analysis and not particularly useful for an empirical investigation of the forms and mechanisms of business political power.

Our conclusions at this point, however, have begun to move beyond our main aim. Our central argument is that the power of firms and their owners is mediated substantially through political partisanship and through the electoral financing organization of business. For both firm taxation and tax redistribution, politics and elections matter.

Appendix

The Model

The model of the relationship between asset concentration and investment and economic growth, estimated with ordinary least squares regression (with a lagged endogenous variable) is:

$$\begin{aligned} AC = \underset{(1.48)}{1.74} &+ \underset{(10.20)^{*}}{.78AC_{t-1}} \\ &+ \underset{(2.24)^{**}}{.18I_{t-2}} + \underset{(3.11)^{*}}{3.11\ GDP_{t-1}} \end{aligned}$$

$\hat{R}^2 = .88$; Durbin's $h = 1.41$, n.s.; $N = 32$;

where: AC (asset concentration) = assets of Fortune 100 industrials as percentage of total plant and equipment capital stock; I (investment) = change in capital stock; and GDP = rate of growth of real gross domestic product. *T*-ratios are in parentheses.

Correlation Matrix of Variables (32 observations)

	Balance	Bowles-Gordon-Weisskopf	Concentration$_{t-1}$	Gross Domestic Product (GDP)$_{t-1}$	Investment$_{t0}$	Investment$_{t-1}$	Lobby	PAC-Corp.	Party (D)	Year
Balance	—									
Bowles-Gordon-Weisskopf	.51	—								
Concentration$_{t-1}$	-.15	-.69	—							
GDP$_{t-1}$	-.03	.19	-.08	—						
Investment$_{t0}$	.16	-.07	.40	.55	—					
Investment$_{t-1}$	.21	-.18	.47	.17	.64	—				
Lobby	.13	.32	.07	-.16	-.05	.35	—			
PAC-Corp.	-.55	-.75	.12	-.18	-.14	-.13	-.54	—		
Party (D)	.31	.27	-.12	.33	.34	.05	-.24	-.08	—	
Year	-.68	-.92	.47	-.15	-.02	.03	.30	.87	.20	—

Notes

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1. This argument, however, makes the broad assumption that firms have strong interests in lower taxes and continually attempt to exercise power to that end. This assumption might not be sound if, for example, firms had other goals of equal importance (e.g., securing trade or employment legislation) and chose to use political capital to that end. Individual firms might also conclude that working to lower tax rates could invite political reaction from other interests.

2. Using the Bowles, Gordon, and Weisskopf measure of class power as a dependent variable in an ordinary least squares model, the coefficient of the asset concentration measure was $-.133$ (-4.08^*, t-ratio), with no serial correlation and an adjusted R-squared equal to .97.

3. We used IRS data reported by the Bureau of Economic Affairs and published by the Federal Reserve Bank's *Flow of Funds* (1989b) because this measure is what the government actually collects from corporations at the end of the year. One problem with the measure is that beginning in 1980, the treatment of the reinvestment of the foreign affiliates of U.S. companies changed. The profit figures began to include the reinvested earnings of these foreign firms, thereby introducing a non-U.S. component to the reports of taxation. The new numbers will be less sensitive to changes in U.S. economic and political conditions, since they include the effects of foreign economies and policies. We used the original U.S. data where possible (for details of the data collection changes, see "The National Income and Product Accounts of the United States: An Introduction to the Revised Estimates for 1929–1980," *Survey of Current Business*, December 1980, pp. 1–26). When the updated data are used, the basic findings reported here remain (though particular effects are a bit weaker).

4. The second redistribution model is a capital gains tax redistribution model, wherein the capital gains income taxes paid by individuals is divided by the wage taxes paid by individuals. The capital gains data are provided by the Office of Tax Policy of the U.S. Department of the Treasury. The results are not reported here, but the table is available from Quinn.

5. Interview with Dr. Anne Bedlington, former chief statistician for the Federal Election Commission (FEC), 6 April 1990. Common Cause does report some campaign expenditure data for the 1971–76 election cycles; but these data are not comparable to the FEC data, since the Common Cause and FEC definitions of businesses differ substantially. In consequence, we cannot splice Common Cause data to FEC data in a time series analysis and expect valid and reliable parameter estimates.

6. In getting a .89 bivariate correlation, we used data from 1976 to 1988. We compared the election cycle campaign contributions from corporate-sponsored PACs as a percentage of all campaign contributions (taken from Ornstein, Mann, and Malbin 1990) to the percentage of corporate sponsored PACs as a percentage of all PACs during the corresponding election cycle.

7. The measure does have some limitations. First, only those who seek to influence Congress directly are required to register. Thus, organizations who seek to influence public policy through public appeals to members or through political advertising need not—and usually do not—register. Second, the data are not fully categorized by Congressional Quarterly after 1971 and are not presented with summary statistics. We made efforts to construct a valid and reliable measure (e.g., two separate and independent counts); but it may have resulted in an overcount of business lobbyists and an undercount of citizen groups. To our knowledge, this measure of business's governmental activity has not been used before in time series research, possibly due to the great difficulty in creating it.

8. The *Statistical Abstract* measure is not used here for several reasons. First, the data refer only to manufacturing corporations, whereas we are examining the universe of nonfinancial corporations. Second, the table was discontinued in the *Statistical Abstract* after 1985. Finally, the sampling procedures used by the Federal Trade Commission (the agency previously responsible for the table) were changed after the 1973–74 data period, making the pre-1974 and post-1974 data incomparable. We thank Glen King of the Commerce Department for his assistance concerning this measure. The relationship between Jacobs's measure and ours is a statistically significant one:

$$\begin{array}{llll} \text{Con} = & 35.53 & + & .53\ (\text{One}) \\ & (7.22^{*}) & & (2.08^{**}) \end{array}$$

Model = AR1; Adj. R-squared = .65;
DW = 1.52; Bivariate correlation = .58;
Con = *Statistical Abstract* measure of concentration;

One = Fortune 100 assets as a % of corporate capital stock.

9. The form of the additional specification test we estimated is $Y_t = \beta_0 K + \beta_1 Y_{t-1} + \beta_2 X_t + \beta_3 X_{t-1} + e_t$. If $\beta 1 * \beta 2 = -\beta 3$, and $\beta 3 \neq 0$, then serial correlation is present, Durbin's *h* notwithstanding. See Griliches 1967 for a discussion and Jacobson 1990 for an application.

10. The observed dependent variable is regressed on exogenous variables (lags of the independent variables). Based upon this equation, the predicted value of the lagged endogenous variable is estimated (*Yt − 1) and then entered as a replacement for Y(t − 1) on the right-hand side of the equation. The procedure then requires the use of an autocorrelation model, after which the regression diagnostics (e.g., DW) become interpretable again. The use of this procedure (once adjusted for first- or second-order serial correlation) serves the purpose of overcoming the initial problems owing to serial correlation (as determined by the Durbin's *h*-test or by an alternative test when Durbin's *h* cannot be calculated; for a discussion of the alternative procedure for testing for serial correlation, see Ostrom 1978, 54).

11. See n. 3.

12. As shown in the Appendix, the asset concentration measure and the Bowles, Gordon, and Weisskopf measure of business power are correlated (−.69); the Bowles, Gordon, and Weisskopf measure is correlated −.75 with the PAC measure; and the PAC measure is correlated −.54 with the lobbying measure. This suggests multicollinearity when these pairs are in a model, perhaps causing us to accept the null hypothesis of *no effects.* To guard against that possibility, we report the models with different specifications, including one model with all the variables in it. Another variable sometimes included in time series models to correct for omitted variables is a time trend variable. Here, however, time is strongly correlated with the Bowles, Gordon, and Weisskopf measure (−.92) and business PACs (.87) and moderately correlated with asset concentration in the Fortune 100 (.47).

13. See n. 12.

14. The difference between Jacobs's findings and ours might arise from differences in testing and modeling. The dependent variables differ: our business tax models use actual corporate tax payments by nonfinancial corporations divided either by pretax corporate profits, total corporate assets, or individual tax collections; Jacobs's model uses, as a measure of manufacturing firm taxation, a measure created by Holland and Myers (1980). Some independent variables differ, as do the time frames (1954-87 vs. 1948-78). See Tables 2, 3, and 5 and Jacobs 1988, 866-67 for a comparison of the models.

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MORAL EDUCATION AND THE "TIE THAT BINDS" IN LIBERAL POLITICAL THEORY

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I examine the search for a "tie that binds," or "core" values, in liberal political theory, specifically Rawls's recent arguments, and in proposals concerning moral education in the public schools. Both Rawls and the proponents of moral education appeal to consensus or shared values, but the search for core values in both theory and practice is only partly successful. Specifically, this search is misguided insofar as it does not reflect how values are embedded in specific institutions and practices. The various forms of moral education in the public schools, both implicit and explicit, illustrate a consensus about a range of moral and intellectual virtues that is broader and more complex than arguments for core values allow. Comparing arguments concerning core values in political theory and moral education suggests how liberal political theory might deal with questions of consensus, justification, and the task of political theory generally.

Blest be the tie that binds
Our hearts in Christian love;
The fellowship of kindred minds
is like to that above
. . . .
We share each other's woes,
Each other's burdens bear,
And often for each other flows
The sympathizing tear.
—John Fawcett

This hymn, written in 1782, celebrates the "tie that binds" an ideal Christian community. Whatever differences there might be among its members, they share a common faith, the "fellowship of kindred minds," and a concern and love for one another ("share each other's woes, Each other's burdens bear"). The virtues of Christian faith and charity define and guide the community. They are simple, yet powerful, values that override other differences between or among individuals. The separate identities of individuals are less important than their shared faith and love as Christians.[1]

Ideals expressed in song often bear little relation to reality. The rise of liberalism as an ideology is explainable in part by the fact that Christian communities failed to find or maintain any such tie. The resulting religious wars and persecution suggested that if there were to be ties at all, they had to admit a fair measure of pluralism. In keeping with this tradition, some contemporary liberals have suggested that liberalism is characterized by the limits to, or absence of, common views of virtue. Sometimes this idea is expressed in terms of neutrality, that is, that liberal societies are or should be neutral among individual conceptions of the good (Ackerman 1981; Dworkin 1978; Thigpen and Downing 1983). Another way of expressing this notion is that liberal ideology is less concerned with finding some common ground among individuals than providing a modus vivendi among them (Larmore 1987). However expressed, there is the sense that liberalism is distinguished by characteristics or principles that preserve the separate identities of individuals and avoid or limit anything analogous to the Christian tie that binds.

It is just this tendency in liberalism that has inspired the recent "communitarian" critique of liberalism (Barber 1984, 1988; Bellah et al. 1986; Lasch 1987; MacIntyre 1981, 1988; Sandel 1982). The communitarians are far from a unified group, and it is not always clear whether they are offering a clear alternative to liberalism. Nonetheless, their critique applies to both liberal theory and the political features of liberal societies. Liberal theory, it is claimed, is excessively individualistic and insufficiently historicist. It implies a peculiar view of the self, one divorced from social relations that might constitute it. Liberal claims to neutrality misunderstand the idea of community and render liberal theory unable to account for—let alone recommend—virtue within liberal society. Moreover, liberalism is said to misunderstand claims to rights, treating them as transcendent principles rather than "situated" products of the historical and contingent features of liberal communities. The political indictment is that liberal societies are excessively individualistic and that there is a need to develop or restore some sense of community. The emphasis on individual autonomy and rights has damaged public discourse and created or exaggerated many social problems. Whatever form it takes (and whether these forms are consistent with one another), the critique calls for more "communitarian" theory and practice in liberal societies.

Liberals have offered both replies to and criticism of the communitarian position (Feinberg 1987; Gutmann 1985; Kymlicka 1988; Thigpen and Downing 1987). In what is an apparent shift from his previous position, John Rawls has recently claimed that his two principles of justice (for each person to have "an equal right to a fully adequate scheme of basic rights and liberties . . . compatible with a similar scheme for all" and for social and economic inequalities to be "attached to offices and positions open to all under conditions of fair equality of opportunity" and "to the greatest benefit of the least advantaged members of society" [1971, 60, 302; 1985, 227]) are "political, not metaphysical" (Rawls 1985, 1987). They represent "the kernel of an overlapping consensus" in the liberal democratic tradition.[2] There might be any number of reasons for this shift in Rawls's reasoning away from a philosophical to a political interpretation of his theory. Consciously or not, the shift implies concessions to the communitarian critique. Rawls's recent interpretation of his theory may be seen as trying to discover what the liberal democratic "community" is. Instead of asking philosophical questions about the self or justice, we are instead invited to ask what binds and underlies the complex tradition of liberal democratic thought. The two principles, it is claimed, are or could be agreed to by "each of the opposing comprehensive moral doctrines influential in a reasonably just democratic society." This acceptance is valuable because it serves the "task of political theory" in a liberal society; namely, it provides the basis for a "shared, workable order" among groups that hold different and even incommensurable positions on philosophy, morality, and politics. Even if partisans of these positions interpret the two principles in their own way, they can still agree "in principle" to them. Thus, the test these principles need to meet is that of being an uncontroversial description of common, basic elements in liberal thought. Meeting this test permits them to serve a practical function as a "political charter" for liberal societies and a "shared basis" for political discussion within them.

As political theorists struggle with the notion of what values, if any, bind or should bind liberal societies, some politicians are confident of the existence of, and consensus on, basic values. Recently, there have been calls for explicit moral teaching in the public schools. The former secretary of education, William Bennett, has been the most visible and vocal spokesperson for an explicit, didactic discussion of values in the classroom. Undeterred by "time-worn doubts" about whose values will be taught and how, Bennett and others have repeatedly offered a list of consensual values that the schools should promote:

> In defining good character we should include specific traits such as thoughtfulness, fidelity, kindness, diligence, honesty, fairness, self-discipline, respect for law, and taking one's guidance by accepted standards of right and wrong rather than by, for example, one's personal preferences. . . . There is a good deal of consensus among the American people about these character traits. . . . Not only is there a consensus among the American people on the elements that constitute good character, most Americans want their schools to help form the character of their children. (Bennett 1986, 2–3)

This kind of appeal is not confined to the right of the political spectrum. For example, Mario Cuomo has also suggested that schools should promote allegiance to fundamental, or "core," values: "At the core of every society is a set of moral values, a code of behavior, a credo. . . . Even here in our uniquely free society where diversity of belief is protected . . . there is a rough—but clear—national understanding of what is right and wrong, what is allowed and forbidden, what we are entitled to and what we owe . . . real, tangible, specific values. And we can teach them to our young specifically" (1987, 6–7). There is a remarkable confidence that there are widely shared and easily known values worthy of promotion by public institutions. The only problem is to persuade the relevant institutions—in this case the public schools—to promote them. Consensus on basic or core values is offered as both the rationale for, and substance of, educational policy.

I shall examine the search for a tie that binds, or core values, in liberal political theory, particularly Rawls's recent arguments, and in arguments for moral education. There are some similarities between the two, similarities that indicate what is right and wrong with the search for core values in both theory and practice. Specifically, I will argue that this search is misguided insofar as it does not reflect how values are embedded in specific institutions and practices. Basic values are, as the communitarian critique suggests, best understood by locating them in specific historical and institutional contexts—though not perhaps in ways some communitarians might wish. The various forms of moral education in the public schools, both implicit and explicit, reflect consensual values in liberal society. These values, however, are not reducible to some small number of first principles or list of basic values. Rather, there are several strands of moral and intellectual virtue that the schools aim to promote, a range of virtues that is broader and more complex than arguments for core values allow. Comparing arguments concerning core values in political theory and moral education yields some insights about consensus, justification, and the task of political theory in liberal society.

Rawls: Politics, Consensus, and the Task of Political Theory

Rawls's claim that his two principles are "political, not metaphysical" involves an argumentative strategy similar to that used by the advocates of moral education. Both appeal to widely held, fundamental values in order to pursue a practical purpose. Both avoid providing any fuller (metaphysical?) justification for these consensual values, but for different reasons. The promoters of moral education believe such a justification is available but unnecessary. Philosophical exploration of what we know to be right and wrong is less important than active promotion of basic values. On the other hand, Rawls adopts what might be termed a "pragmatic" skepticism: he doubts the utility of attempting to develop a deeper philosophical rationale for an overlapping consensus.[3] He claims that his conception "deliberately stays on the sufrace, philosophically speaking," meaning that it is independent of "disputed philosophical, as well as disputed moral and religious questions." He seeks to avoid these questions because "philosophy as the search for truth about an independent metaphysical and moral order cannot, I believe, provide a workable and shared basis for a political conception of justice in a democratic society" (1985, 230).

Rawls's appeal to political consensus as opposed to philosophy is based upon both this pragmatic skepticism and a self-consciously historical and practical view of the task of political theory. This task involves a specific type of community and political mission. In a liberal democratic society, political theory must provide a means for dealing with the inevitable "fact of pluralism," the existence of irresolvable philosophical and moral differences. This fact suggests the desirability of (or even the necessity for) a political agreement on basic principles, a political charter that can limit serious conflict over philosophical issues and thereby avoid the imposition of some views through state power:

> This diversity of doctrines—the fact of pluralism—is not a mere historical condition that will soon pass away; it is, I believe, a permanent feature of the public culture of modern democracies. Under the political and social conditions secured by the basic rights and liberties historically associated with these regimes, the diversity of views will persist and may increase. A public and workable agreement on a single general and comprehensive conception could be maintained only by the oppressive use of state power. . . . We must find another basis of agreement than that of a general and comprehensive doctrine. And so, as this alternative basis, we look for a political conception of justice that might be supported by an overlapping consensus. (1987, 3–4)

A political theory appropriate for liberal democratic societies, then, is one that aims at discovering, articulating, and obtaining consensus on the principles that, regardless of other differences, are shared by broad segments of the society—the kernel of an overlapping consensus. Instead of seeking universal principles that can be justified on a philosophical basis ("comprehensive doctrine"), we should seek—and Rawls claims to offer—uncontroversial principles that serve a practical purpose for liberal democratic societies.

Rawls's claims have been, and will continue to be, subjected to close criticism, since they represent a putatively new position taken by an eminent theorist. Without developing a full critique, several potential problems should be noted concerning this political turn. There is the threshold issue of what the practical problem is for which this charter is a solution. Presumably, liberal democratic societies are in danger of the abuse of state power without such a charter. This presumption may at least be questioned, given the resilience of liberal democratic polities in the face of internal conflicts about, and external critiques of, liberal democratic ideology and practice. Moreover, the distinction between *political* and *metaphysical* might also be doubted (Neal 1990); and it may be that Rawls has apparently avoided philosophical issues only by implicitly importing a liberal metaphysics into his discussion. Finally, one might reasonably question why this definition of the task of political theory should be accepted at all. Political agreement is not philosophical argument (Spragens 1988), and political theorizing often implies that our fundamental principles are in doubt and consensual values need to be questioned, clarified, justified, and, if necessary, changed.

Let us, for the sake of argument, temporarily put aside these doubts. Let us assume with Rawls that the discovery and public ratification of some basic principles is valuable and that a liberal democratic society in which such an agreement has been obtained is somehow better off than one without it. Let us also assume that we can separate the political nature and role of these principles from any metaphysical considerations and that agreement on such principles is sufficient warrant for them—in any case all the warrant we are likely to obtain without "oppressive use of state power." In short, let us assume that Rawls has successfully set forth the task of political theory.

The question then becomes whether Rawls's two principles are appropriate to the task as he defines it. Again, a complete critique is impossible here; but there are at least three problems or ambiguities concerning his argument that have interesting parallels in recent calls for moral education. First, it is unclear what practical role the two principles, understood as political, are to play. They may be understood as either guides or boundaries to political debate. Both notions face difficulties, and there is some tension between them. Second, there is an important ambiguity about whether Rawls's claim that his principles are consensual is primarily an empirical ("anthropological") claim or a normative ("rhetorical") one. On either account (or some mix thereof) it is at least questionable whether Rawls's two principles can be claimed to be the kernel of an overlapping consensus or whether other principles might make similar claims to that status. Third, it is doubtful that any principle or small number of principles (Rawlsian or otherwise) can claim this kind of practical power and consensual status. These criticisms produce an interesting, perhaps heretical, possibility: that ordinary politicians like Bennett and Cuomo may provide, at least at first glance, a more plausible approach to finding a tie that binds, a better way of doing the task of theory.

First, in specifying his principles as political, Rawls clearly believes that they will serve some practical purpose. But it is not entirely clear what that purpose is or whether it is consistent with the idea of an overlapping consensus. Perhaps the most obvious, strong view is that these principles serve as guides. They provide good reasons for political proposals and criteria for judging political institutions, "a publicly recognized point of view from which all citizens can examine before one another whether or not their institutions are just . . . by citing what are recognized among them as valid and sufficient reasons singled out by the conception itself" (1985, 229). Alternatively, the principles might be understood, in the weak view, as constraints or boundary conditions on political discussion and institutions. Liberal societies are predicated on allowing the existence of a plurality of conceptions of the good. The pursuit of these varied conceptions of the good is bounded by a notion of right expressed in the two principles: "These conceptions respect the limits specified by appropriate principles of justice" (p. 249). The two principles, as a political charter, establish the broadest terms of cooperation in a liberal society and set the parameters within which the pursuit of potentially incommensurable goods is possible. On either understanding, whatever disagreements remain are dampened because they are discussed on the basis of, or within a consensus on, the two principles "as theorems, as it were, at which their several views coincide" (p. 247).

Leaving aside the questions of whether there is such a consensus and whether the existence of such a consensus justifies anything, the practical value of discovering or achieving this kind of consensus is dubious. On the strong view, if various parties are free to interpret and synthesize the principles as guides in their own ways, the consensus may be more symbolic than real. For example, Rawls notes that the liberal tradition is of (at least) two minds about liberty and equality, with Locke and Rousseau representing contending camps. But if the modern partisans of these positions interpret Rawls's principles in their own ways, their disagreement is not—at least not obviously—lessened; they merely have another language in which to express it. To the degree that these principles lead to conclusions in conflict with their "settled convictions" (e.g., concerning property rights) they might well affirm conviction over principle. Similarly, the weak view is potentially too weak to accomplish the task of political theory. One might agree with Rawls that principles of liberty and equality are in some sense basic, "boundary" principles for liberal democracy but then wonder whether agreement on this status provides the significant political and practical services that Rawls claims it does. These boundaries of right are still open to differing interpretations, and the principles do not provide any guidance for conflicting claims about the good.

Indeed, these two conceptions suggest a dilemma in themselves (Paris 1987). If we take the strong view, the principles are less likely to have the consensual standing Rawls attributes to them. If they really do have some power to determine important political outcomes, they may not be seen as consensual by those whose views lose out. The strong view is thus too strong for consensus. Alternatively, if the principles are seen as weak, the shared basis may be more readily agreed upon; but this provides a largely symbolic sharing—liberals are committed to liberty and equality, somehow understood, and little else. Again, as Rawls notes, each group will interpret them in its own way. Thus, a strong, guiding view is likely to fail to be consensual while the weaker, boundary view is likely to fail to play a significant role in lessening disagreement.

A second problem involves an ambiguity about Rawls's claim that his principles represent the kernel of an overlapping consensus. This claim might be initially taken in an empirical or anthropological way, as reporting what the "tribe" of liberals believes to be its basic, shared principles. Alternatively Rawls's claim might be seen as normative or rhetorical. That is, the two principles are offered as persuasive definitions of the charter of liberal democracies; these are principles that all groups in a liberal democratic society will, upon reflection, find that they should share. Their validity depends upon their power to win the allegiance of different groups who hold "opposing comprehensive moral doctrines." The resulting agreement is in a sense self-justifying but not unreflective or merely arbitrary. There is no common philosophical rationale or foundation for the principles beyond consensus on them by different groups—there cannot be such a foundation, given the "fact of pluralism"—but Rawls does offer many of his earlier arguments as reasons for accepting them as consensual. It is critical to note here that this rhetorical conception is not wholly separate from the empirical one. It would make little sense to offer a persuasive account of charter principles, understood as political, if they bore no discernible relationship to common understandings. The winning of agreement to the proposed charter is the test (and the main justification) of these principles; and presumably they could not meet this test, in more than a symbolic way, unless they had some resonance with existing beliefs. This relationship between empirical and rhetorical consensus seems to be the political form of "reflective equilibrium" (Rawls 1971, 46–53); a balance is sought between actual patterns of belief and some proposed distillation or codification of those beliefs. Nonetheless there remains some, perhaps inevitable, ambiguity about this mix of empirical and rhetorical elements.

In either case, it is not clear that Rawls's two principles do represent the kernel of an overlapping consensus. It seems at least arguable that the public mind, past or present, is in fact better represented or codified by some libertarian principles such as Nozick's views or some form of rule utilitarianism. It is questionable whether the common, egalitarian understanding of Rawls's principles is widely shared. If Rawls were primarily offering an empirical claim for his principles, one might expect some detailed historical or anthropological argument from Rawls for the status of these principles as a kernel. Instead, one finds a more standard philosophical argument—for example, discussions of the original position as a representational device, arguments about moral persons, and so on. This suggests that Rawls is not doing questionable anthropology or sociology; rather, he is engaged in a more rhetorical task of providing a persuasive definition of charter principles. Suggesting that Rawls is engaged in this rhetorical enterprise does not mean he has succeeded at it. As with the empirical approach, it is not clear that Rawls's principles would be able to win widespread or meaningful allegiance or that some other proposed charter principles could not claim to have an equal or greater ability to do so. For example, we might all be persuaded to adopt some version of utilitarianism. Once Rawls opens the possibility that different groups in liberal society can synthesize and interpret proposed charter principles in their own way (i.e., retain many or all of their preexisting allegiances), it is not clear why groups would prefer Rawls's principles to other contenders or vice versa. In short, there are good reasons to doubt that Rawls's principles represent, either empirically or rhetorically, liberal societies' underlying consensus.[4]

Both these criticisms at least allow the possibility that there is some coherence to our tradition and, more important, that its coherence can be expressed in one or a few principles. Actual traditions may be a poor approximation of these theoretical ideals. For example, there may be some deep consensus about the importance of rights but not about their absolute priority over considerations of consequences. Neither of these reflect procedural values that might have a similar status. Perhaps more important, the shared understandings in a liberal society may include virtues that are not deducible from principles such as those offered by Rawls. They may reflect traits of character or attitudes (e.g., toward work or marriage) such as those listed by Bennett and Cuomo. There may even be widely held values that are not clearly liberal. For example, Stout points out that in interviews conducted by Bellah and his colleagues, the interviewers tended to read individualistic commitments into responses that could be equally well interpreted as moralistic, even Aristotelian, views of virtue and obligation (1986, 34–38). It may be that charter principles (if they exist at all) are more numerous and complex than Rawls's argument allows, extending beyond political values or principles to ideals of character and conduct.

These criticisms raise the heretical possibility that the view of basic values offered by Bennett and others may be a better way of approaching the task of political theory than Rawls's approach. If, as Rawls and his communitarian critics seem to agree, it is important to understand how the liberal community defines itself, the open-ended set of values provided by the proponents of moral education seems to be a more accurate portrayal of an overlapping consensus than Rawls's principles. It may be empirically and rhetorically sounder because it cites many of the virtues that are either assumed or ignored in Rawls's account and because it offers a more comprehensive basis for agreement. This would certainly be consistent with a communitarian perspective in that it looks to the contingent, "situated" self-conceptions of the community. Moreover, the view of Bennett and others does not fall prey to being too weak to serve as a basis for prescription; it suggests a range of virtues related to character and conduct and implies a fairly strong, prescriptive understanding of them. Finally, if the task of theory is primarily practical—to develop a working, stable, consensual agreement based upon a tradition—then didactic education in a "credo" would seem to be an obvious way of fulfilling the task of theory. If this is the case, we might simply tell the theorists to get out of the way and let the politicians and educators articulate and teach core values. Political theory might help clarify these conceptions, but these would be refinements of the more basic task of simply asserting and teaching what we already know. It is, of course, not as simple as this; and an examination of the current push for moral education will indicate why and suggest some implications for both theory and practice.

Moral Education: Moral and Intellectual Virtues in the Schools

The recent proposals for moral education may be seen as part of a second wave of the reform movement that began with the publication of *A Nation at Risk* (D. Gardner 1983). Declaring that the United States was in danger because of a rising "tide of mediocrity" engulfing the schools, the National Commission on Excellence in Education suggested, among other things, stiffer graduation requirements, including more science and mathematics, and longer school days and years. Whatever the merits of the original indictment, the states responded quickly and adopted a number of reforms. Shortly after the original burst of crisis and reform, a second wave appeared that was a logical extension of the first. If we were being soft on academics, then it was important to say what we wanted students to learn. It was important to focus on facts in history; drill and repetition in mathematics; and "cultural literacy," that is, what every literate citizen ought to know (Hirsch 1986). But the assurance that students would learn something specific in school was not enough; they must also be taught to act properly, to conform to the values underlying the culture. Gallup polls consistently indicated that drugs and lack of order and discipline were seen as the most serious problems in the public schools. Explicit moral education was offered both as a response to these problems and as fitting with the broader view that society should promote its culture and values.

Beyond reacting to public perceptions of disorderly schools, the recent push for moral education is also a reaction to curricula, popular during the 1970s, that seemed to emphasize the *process* of making moral judgments rather than their substance. For example, exercises in "values clarification" were popular (Raths, Harmin, and Simon, 1978). These and similar programs often seemed to imply that the teaching of *any* specific values was a violation of student autonomy. Moreover, they often fit neatly with school administrators' pronouncements that the school should be neutral on moral matters. As one district superintendent put it, "Whose version of ethics would we use?" It's outside the scope of our charge to teach morals" (Friendly 1985). The apparent relativism of this approach fairly begged for a strong reaction in the other direction. Instead of worrying about *how* students think about moral matters, it is now argued that we should focus on *what* they think and should make sure they think—and do—the proper things. There is a range of agreed-upon values and virtues, the reformers argue, and these should be promoted by the public schools. We need not worry about indoctrination or imposition because these values and virtues are widely shared by the public. As the tie that binds a liberal society, they need to be promoted and strengthened.

The programs proposed by Bennett and others have the apparent advantage over previous programs (or an approach such as Rawls's) of offering a rich, substantive set of core values and a practical vehicle for promoting them. However, there are also some obvious problems with these proposed programs. First, the rhetoric promoting them is not very specific about their nature and content. It is seldom clear how the "laundry list" of values is to be taught: Will it be treated as a separate subject "like physics" (in Bennett's phrase) or infused within the curriculum? Will it emphasize broadly moral concerns or be focused more specifically on citizenship? How these questions are answered would produce very different programs in moral education (Nelson 1980). Second, even if we had answers to these questions, it is not clear how these values could be taught in a way that would be both nontrivial and noncontroversial. Like Rawls's principles, this list of values may be consensual at the price of being merely symbolic and if more than symbolic, may not be genuinely consensual. Certainly, we can tell children that it is wrong to lie, steal, and so on; but many of our moral problems involve issues where the implications of these basic values are in dispute. For example, it is one thing to teach that murder is wrong and quite another to say whether abortion is murder. It is one thing to list core values; it is quite another to establish a specific curriculum in moral education that does not offend some groups. In a liberal democratic society, the public schools are always open to the charge that they are imposing values upon some groups, as the ongoing battles over prayer and textbooks illustrate. A listing of values may not constitute a specific consensus about what is to be taught under the banner of moral education.

These are not killing objections. The proponents of moral education can reply, with some justice, that they are better able (than, say, Rawls) to articulate a rhetorical consensus because the core values to which they appeal are, as a matter of fact, widely shared and similarly interpreted (that is, there is very little gap between empirical and rhetorical consensus). Therefore, the specific content of these programs can be worked out, given the broad consensus on basic values; the potential for controversy is lessened because of this consensus; and any residual conflict is a small price to pay for an important program. Whatever the merits of these objections and replies, there is a more important issue concerning moral education in the public schools; namely, the reformers' critical depiction of the state of moral education in the public schools as based on programs in values clarification or on school administrators' claims of neutrality in moral matters is something of a caricature. Contrary to being neutral about questions of value and virtue, the schools do engage, often necessarily, in moral education. If we look more carefully at what the schools actually do—or at least attempt to do—rather than appealing to an abstract political theory or a list of values, we can see the schools promoting, albeit imperfectly, a range of moral and intellectual virtues. We can discover several strands of virtue "situated" in a variety of ways in the public schools, reflecting some of the consensual values of liberal society. That the schools engage in moral education in these ways with relatively little controversy indicates that there is some consensus about certain values or virtues as well as about the role of the school as a moral educator. Considering some of the virtues embedded in school curricula and practice may provide a more accurate picture of both what the consensual values (the ties that bind) in a liberal society are and how they function within one of its institutions.

To put the matter another way, if the task of political theory is to discover common, basic values (to move from metaphysical foundations to political consensus), one might appeal, as Rawls does, to a few basic ideas underlying a tradition of discourse. Alternatively, one might, as Bennett and others have, list a range of shared values with which "no one would argue,"—at least not in the abstract. The communitarian critique suggests another alternative, one that might be termed more "ethnographic" (Walzer 1983). Instead of developing, by philosophical analysis or appeals to consensus, a list of core values, one might look at how communities and institutions promote (or fail to promote) consensual values, including notions of virtue.[5] Examining, even in a cursory way, the variety of virtues that the schools attempt to promote can provide some sense of the nature and range of core values of liberal society. Specifically, the schools "teach morals" to a greater or lesser extent (1) in health classes, (2) through the "hidden curriculum," (3) in the course of regular subjects, and (4) by promoting critical thinking. The first two are primarily concerned with promoting moral virtue, while the latter focus on the development of intellectual virtue.[6]

Moral Virtues: Hobbesian Health Class and the "Hidden Curriculum"

The call for schools to teach morals in a didactic way—even "like physics"—fails to note that there is already such a course in public schools. It is called health class. It is not usually thought of as teaching morals save when controversies arise—for example, relating to sex education. When we think of health class, we almost automatically think of sex education. This is both predictable and unfortunate. It is predictable because health classes attract attention when they consider issues that spark controversy. It is unfortunate because it distracts us from the fact that health class is the closest thing to the style of didactic moral education proposed by Bennett and others. For the most part health classes promote values that are basic, widely shared, and uncontroversial; therefore, teaching these values in a didactic way almost never strikes anyone as violating students' rights.

What are the values taught in health class? Without pushing the analogy too far, it is reasonable to call them "Hobbesian." First and foremost, they relate to one's physical well-being. Students learn how to take care of the irreducibly distinct, physical self. The primary concern of health class is the care of one's body, and the body is spoken of as a kind of mechanism. Second, the topics covered in health courses appeal directly to self-interest. Students are presented with the consequences of certain kinds of behavior and exhorted, ultimately, to take steps to preserve themselves (Redican, Olsen, and Baffi 1986, chaps. 14, 15; Seffrin and Torabi 1984).[7] Where health class involves didactic teaching regarding these prudential concerns, such as avoiding drug and alcohol abuse, there is no controversy concerning the teaching of moral virtue(s). Indeed, we demand that the schools engage in teaching these because they are presumed to reflect important, common values.

It is tempting to suggest—a whole school of theorizing has done so—that the core values of liberal society are Hobbesian. (A significant thrust of the communitarian critique suggests that is precisely the problem with liberal societies.) The values taught in health class are really only core values in the weak sense of being boundary principles. Liberal society, and perhaps any society, is prepared to acknowledge and encourage moral virtues related to self-preservation. We do not question the right of individuals to preserve their physical well-being and are prepared as a society to encourage young people, in a didactic way, to do so. We are not prepared, except in very rare circumstances (warfare), to suggest that individuals ought not to pursue their basic physical health.

Virtues related to self-preservation and satisfying legitimate physical needs are not, however, a complete picture of moral virtue. Beyond bodily needs, we are unlikely to treat Hobbesian virtues as any more than a partial constraint on other claims to virtue. In political theory generally, the attempts from Hobbes onward to draw stronger political or moral inferences have encountered tremendous difficulties. In the case of health class, recent curricula have tended to try to broaden the range of issues to family and interpersonal relationships. For example, one model curriculum suggests that the "decision-making skills" developed in dealing with personal health may be applied to other interpersonal relations; similarly, it is often suggested that students recognize in others the stresses and problems they find in themselves (Quest International 1988). (This too reflects a kind of Hobbesian approach in its reliance on introspection.) But these extensions of the basic lessons of health class are only carried in directions that are modest and unproblematic. The primary goals of the health class are to remind students of their (self-)interest in bodily health and persuade them to act in accord with moral virtues related to prudence (self-control, moderation). While these moral virtues are not the only traits of character prized in a liberal society, they are sufficiently widely held and important enough for schools to promote them in a didactic way.

Perhaps a more powerful way in which the schools engage in moral education is through what is commonly called the "hidden" or "implicit" curriculum. At a minimum, this includes the rules and customs of the school as an organization. The school might not offer morality as a subject (beyond health class); but it inevitably teaches by example in how the school is organized, how rules are set and enforced, and how personal relations are conducted between and among the various groups within the school. Much of the literature on "effective schools" notes the importance of the "ethos" of good schools, the safe and orderly learning environment, and the culture of success that such schools create. The terms *hidden* and *implicit* are not entirely apt, since it is important to make clear to students what is expected and why, giving students a stake in creating reasonable behavior in the school (Sizer 1983, 120–31). Explaining the rules and the reasons for them is not typically a matter of referring to basic moral principles. Rather, it generally presumes the legitimacy of the school's purposes and tries to enlist—and, if necessary, demand—student cooperation. Although school administrators often deny that this posture is "teaching morality," it is clear that the schools routinely provide moral guidance concerning civility, tolerance, and numerous other virtues as part of meeting the basic needs of the organization.

There are also many instances in which the schools more explicitly encourage or even require students to participate in activities that are to be valued or praised. The daily rituals of the school (the pledge to the flag) and extracurricular activities promote political and personal values and traits of character. Some schools are experimenting with more self-conscious promotion of student cooperation on an everyday basis, and many states and districts also have community service requirements. What is crucial about these broader aspects of the hidden curriculum is that they go well beyond the individualistic lessons of the health class and the necessary rules of school order. They provide instruction of a sort in the list of core values listed by Bennett; they suggest noncontroversial virtues related to traits of character and citizenship. As one observer rhetorically asks, "Who would raise a question about anti-litterbug posters in the hall or pictures of Martin Luther King on the bulletin board? . . .

What about participation in the neighborhood cleanup campaign or afterschool visits to the home for the elderly? Would anyone object to these?" (Jackson 1989, 97–98). This list could be extended in any number of directions. To be sure, the encouragement of concern for others and public-regarding behavior may not be programmatic in the way that the ordinary academic curriculum is; nevertheless, there are moral lessons being taught that reflect shared values, including many of those found on Bennett's and others' lists.

Studies of political socialization have often claimed that schools play an important role in transmitting and reinforcing common, dominant political attitudes (Hess and Torney 1967; Jennings and Niemi 1974; for a recent discussion see Westholm, Lindquist, and Niemi 1990). Discussions of the hidden curriculum suggest that this is only a part of a broader set of moral and political lessons offered by the schools. The variety of school rules and practices, explicit or implicit, are also significant for political socialization broadly understood. At the same time, one should be cautious about ascribing too much—for good *or* ill—to the hidden curriculum. It is not clear exactly what it is, and it is odd that something hidden is often talked about as if it is well understood. Many observers commenting upon it have seen quite different lessons being taught. Powell, Farrar, and Cohen (1985) suggest that the schools have little or no moral impact beyond providing another "consumption experience" for young people; the schools, like shopping malls, have an unfocused, consumer-oriented character. Merelman (1980), on the other hand, argues that the hidden curriculum expresses organizational needs that are authoritarian but that conflict with the schools' attempts to encourage the development of independent judgment and democratic character. Still another view is offered by Goodlad (1983), who claims that the emphasis on individual performance encourages individualism and moral insensitivity. Thus, the hidden curriculum has been variously characterized as strong and weak, authoritarian and individualistic. (For a brief review of more general theories of the hidden curriculum, see Lynch 1989, chap. 1.)

It is not crucial to resolve these debates. Many of the competing claims about the hidden curriculum can be consistent with one another; in some ways the school is neutral, in others authoritarian, and so on. The key point here is that many of the basic rules of the organization and the larger lessons taught in the hidden curriculum reflect consensual values. Although there have been conflicts about the rights and powers of the schools, liberal society has typically been generous in allowing schools to establish norms and expectations for behavior as part of the educational enterprise. Even the strongest advocates of students' rights recognize the schools' legitimate claims on minimal standards of self-discipline, cooperation, respect for others, and tolerance. Similarly, formal or informal activities that encourage public-regarding behavior provide another way in which the schools promote moral virtues. These too have generally not met with much opposition, because they reflect widely held values of the local or larger community; and some observers have suggested that these moral lessons are as important as academic ones.

It may be asked whether the schools are doing enough of this kind of thing or whether they are doing it in a sufficiently systematic way. It is not clear whether a programmatic approach in this area would be more effective. For example, the current pattern of moral instruction is probably more effective than a more direct preaching of some specific values. Exercising moral virtue is not simply a matter of intellectual pronouncement and assent; moral virtue is not simply derivative from intellectual virtue, as a didactic program might imply.[8] Practicing these virtues, having them become matters of habit and custom, is more likely to be meaningful to students. More important, these activities are probably safer from critical scrutiny because, besides the fact that they reflect noncontroversial values, they are not offered in a systematic, visible, and didactic way that would invite opposition. They are therefore less open to the charge of indoctrination because they have a dual protection of being noncontroversial and not highly visible. Failing either test risks controversy and challenges to the schools' often professed (and somewhat disingenuous) posture of neutrality. However, even where schools develop more explicit and visible programs aimed at encouraging students to develop regard for others, they are likely to be safe from challenge to the extent that they do not promote controversial moral stances. For example, one experimental program, the Childhood Development Project, institutionalizes students' helping one another in a variety of ways without provoking community opposition (Schaps 1986). Though formal and explicitly moral, this program has broad community support because the values it promotes are basic and widely shared; it is clearly possible that such action-oriented programs could be viable and effective.

It is also important to note that the values or virtues expressed in the hidden curriculum and, to a lesser extent, those taught in health class do not function in the same way that Rawls's proposed charter apparently would. They are not "theorems" within a coherent set of intellectual principles from which guidance can be deduced. Rather, they are more like "labels" that mark off situations and organizational routines, matters of habit and custom, and categories of concern. Educating students regarding these virtues is less a matter of providing principles and deriving consequences intellectually than of getting students to recognize instances in which certain behavior is desirable and to act accordingly—moderation or abstention in physical matters, understanding the rules of the school as an organization, seeing and taking opportunities for public-regarding behavior. The validity and importance of the schools' activity here is largely taken for granted because the values and virtues being promoted are widely held to be legitimate, both in themselves and as a proper aim of public education. The overlapping consensus here does not concern theorems but agreements about these values and virtues and the appropriate role of the school in providing instruction, in word and deed, concerning them.

Intellectual Virtues: Academic Values and Critical Thinking

Health class and the hidden curriculum are both instances in which the schools emphasize moral virtues. The goal is action, proper behavior. In both instances, having—or at least understanding—the right reason may be part of the instruction, but the emphasis is on action, not thought. The failure to act in accord with these virtues is often immediately visible and problematic; alcohol and drug abuse, disorder and violence typically require some organizational response from the school. Proper reasoning may be desirable, but proper action is by and large required. At the same time, schools are also charged with promoting intellectual virtue.[9] Developing the ability to categorize moral concerns, consider relevant evidence and argument, and understand conflict and ambiguity are also part of moral education properly understood and are arguably core values in a liberal society. One of the failings of values clarification was its apparent discounting of intellectual virtue in favor of the expression of preference or desire. The seeming emphasis on unregulated opinion, a substantively empty process of moral "reasoning," prompted calls for a more prescriptive moral education.

The difficulty, of course, with the reformers' proposed emphasis on didactic instruction in core values is that it may encourage an uncritical understanding of these values. In its own way a didactic approach also deemphasizes the importance of intellectual virtue and thereby neglects other values that are also basic and consensual. This is an especially significant omission in a liberal society, given that individuals will routinely meet others with different values or who interpret core values differently. As Horowitz notes, "Moral education for democracy entails the ability to learn how to absorb information about people with different sentiments, values, and interests. This naturalistic vision is the opposite of a notion that moral education is a matter of 'our' teaching 'them' right values" (1980, 16–17). Instead of simply telling students what is right and wrong, there should be an ongoing dialogue about the different ways in which moral virtues and political commitments in our society may be interpreted and understood, weighing evidence and reasons for and against conflicting, yet plausible, positions.

As in moral education generally, there is some question about whether intellectual virtue—moral judgment, critical thinking—should be treated as a separate topic or infused into the regular curriculum. Certainly, academic subjects are a natural vehicle for teaching moral values and for practicing the intellectual virtues associated with moral judgment. Rather than learning a separate litany or catechism of basic values, students can find moral lessons in their regular subjects. These lessons do not require immediate action by students but promote thinking about moral categories and concepts. As Ravitch puts it:

> The answer for demands for character development in the schools lies not in the creation of new courses but in recognizing that the school already has powerful resources. . . . Science, properly taught, teaches children the values that are embedded in scientific inquiry: honesty, openmindedness, critical thinking, and the capacity to withhold judgement in the absence of evidence. . . . Literature is a potent vehicle for questions of social and personal values. . . . History provides limitless prospects for the study of values and ethics. It is a living laboratory in which to consider the relations of ideas, actions, and consequences. . . . As humanistic studies, literature and history inevitably evoke questions of value and inspire questioning about the nature of the good society, the moral attributes of a good life and the qualities of character that awaken our admiration or elicit our contempt. (1984, 22–23)

In using the regular curriculum for moral education, immediate action is less important than the cultivation of intellectual awareness. We encourage students to become aware of the value of open-mindedness, to study the relations of ideas and consequences in history, to consider what we admire and abhor. Unlike health class or the hidden curriculum, the goal is to produce not immediate action but a kind of intellectual orientation, the cultivation of habits of mind that will then presumably produce a more responsible person and citizen. However, it is also important to note that these intellectual virtues are not independent of the moral virtues just discussed. Rather, they involve the ability to see and categorize moral issues, to give relevant reasons and persuasively argue about substantive moral concerns. Intellectual virtue does not guide moral virtue, rather the two are constantly intertwined and interacting.[10]

A good example of this approach is a proposed curriculum for values education in New York. The New York State Education Department (1987) has proposed, in all subject areas, guidelines for syllabi to address value issues. For example, in the sciences the proposals mention the values implicit in research ("valuing truthful reporting") and the implications of science and technology for society. The state proposals emphasize the intellectual skills related to moral judgment rather than emphasizing a "right" answer. Of course, there are substantive, shared values involved (honestly, self-discipline) and the state syllabi reflect these. But the explicit assumption underlying the proposals is that most social problems involve differences in how shared values are to be applied. Students therefore need to develop reasoning skills to help them deal with the intellectual task of applying and interpreting shared values. These skills, however, are not treated as abstract reasoning or an academic exercise; the reasoning skills associated with moral and civic values are not a guarantee of "right" answers. Students must also recognize the possibility of reasonable disagreement, anticipate the inevitability of political conflict, and understand how political processes can resolve these problems within a broader set of social and political rules and institutions: "Even when all sides in a dispute over the application of an accepted civic value to a particular instance are being reasonable, they may still disagree over this application. . . . Our society has procedures for making decisions in such cases" (New York State Education Department 1987, 2).

Recently, there has been some discussion concerning whether critical thinking skills can be defined and taught apart from particular subject areas (Resnick 1987). Much of this debate is an outgrowth of new speculations in cognitive psychology about the nature of intelligence (H. Gardner 1983; Sternberg 1986). On the basis of these theories or on an ad hoc basis, a number of experimental programs and curricular packages for teaching "critical thinking" have been developed, ranging from computer-assisted instruction to discussion of the great books (Baron and Sternberg 1987). For present purposes, it is not necessary to sort through the debate between an "infused" or "separate" approach. Rather, what is interesting about both is that they illustrate a consensus concerning intellectual virtue(s) in a liberal society. The discussion revolves around the nature of these critical faculties and the most effective way to promote them rather than around whether they are valuable capacities. It is presumed that open-mindedness, sensitivity to evidence and argument, reflective judgment, and so on are intellectual virtues and core values worthy of promotion by the schools. Admittedly, the schools do not do nearly enough to promote these virtues. Most field studies of the schools find the classroom atmosphere flat; material is covered without engaging in dialogue, or, worse, potentially controversial material is consciously avoided. Nevertheless, these findings are typically accompanied by suggestions for reform that would promote critical dialogue, suggestions that can be made confidently because they reflect basic values relating to intellectual virtue.

To summarize, if we look closely at the various ways in which the schools engage in moral education, we find them promoting (albeit imperfectly) a range of virtues that reflect core values. Some curricula and practices involve moral virtues concerning right action. The most explicit of these are Hobbesian virtues related to one's bodily needs. These are given a didactic treatment. Though less explicit, moral virtues communicated through the hidden curriculum derive their legitimacy from organizational needs of the school and their reflection of widely shared standards of civility and cooperation in social life. Regarding intellectual virtues, the curriculum itself, properly taught, promotes these virtues and provides practice in thinking about and categorizing moral concerns; programs in critical thinking may serve the same purposes. Again, many schools do too little, if anything, to promote these virtues. Disorganization, discipline problems, the teaching of subjects in a rote manner rather than to promote critical understanding are all familiar criticisms of the schools. At the same time, there are also many schools in which the range of moral and intellectual virtue is encouraged. The success of these schools in actually promoting these several strands of virtue is less important than the fact (as indicated by the ongoing criticisms of the schools) that few would disagree that these are virtues and values the schools should promote. Insofar as the schools provide a practical means for promoting agreement on widely shared values, the core values they embody and the means for promoting them are richer, more complex, and more varied than the list provided by Bennett and Cuomo and certainly than Rawls's two principles. Imperfectly realized though they may be, these values are arguably a more complete and practical representation of an overlapping consensus.

The Ties that Bind and the Tasks of Political Theory

It has been assumed throughout this discussion that Rawls has correctly defined the task of political theory: to discover, articulate, and obtain agreement on basic values. Given this assumption, three general points have been made above. First, Rawls's principles are not fully adequate to the task of theory as he defines it. Their practical function is ambiguous, their consensual status is dubious, and it simply seems that core values in liberal society are more numerous and complex than Rawls allows. Second, through reform proposals for moral education based upon core values appear to fulfill the task of theory better, they, too, face difficulties. They are somewhat vague about how the values they cite are to be taught and interpreted in practice. More important, they fail to take note of the variety of ways that schools do engage in moral education. Third, a glance at these forms of moral education reveals a variety of moral and intellectual virtues that are promoted, though imperfectly, by the schools. These virtues can be promoted precisely because they reflect consensual values ranging from Hobbesian prudence to intellectual virtues.

This analysis has further implications for political theory and moral education in the public schools. Regarding the former, it suggests an alternative to Rawls's approach on issues of consensus and justification and therefore to the task(s) of political theory. To oversimplify a bit, Rawls does not take his political approach to its logical conclusions, because he retains certain philosophical ("metaphysical") expectations about consensus and justification. If we consider consensus and justification as rooted in specific contexts and institutions, Rawls's expectations can be shown to be too constraining. They are not consistent with a more thoroughgoing political approach that better matches political reality in a liberal society and may provide a better way of fulfilling the task of theory as Rawls defines it. With regard to moral education, the previous analysis suggests a more self-conscious pursuit of the forms of moral education within the schools, as well as skepticism about the possibility of a theoretically guided program of moral education like Kohlberg's and milder skepticism about programs like those proposed by Bennett, Cuomo, and others. Taken together, these arguments suggest tasks for political theory that incorporate elements of the communitarian critique without rejecting liberalism.

Consensus and Justification

In moving from a metaphysical to a political understanding of his principles, Rawls seeks to avoid "disputed philosophical, . . . moral and religious questions."

He eschews some broader philosophical rationale for his principles in favor of seeking a (political) consensus on them. These consensual principles can then provide a reference point for political discussion and evaluation; political justifications must at least be consistent with these principles, if not derived from them. Once framed and agreed upon, they provide the practical services of structuring political debate and militating against the arbitrary use of state power, thereby fulfilling the task of political theory.

Though Rawls claims to be advancing a political account of his principles, he retains what might be called "theoretical" (or even "philosophical" or "metaphysical") expectations about consensus and justification in a liberal society. Consensus in a liberal society consists of a relatively small number of principles ("theorems") that guide or bound political debate. Particular commitments, policies, practices, and institutions are fully justified when they can be shown to be consistent with—if not required by—these principles. Other arguments or principles that might be brought to bear must function within a framework set forth by these "theorems." Similarly, justifications must be theoretically grounded, and the relevant, consensually established principles have the characteristics of a good theory. They are parsimonious, coherent, and comprehensive; Rawls offers two (*parsimonious*), lexically ordered (*coherent*), principles underlying the liberal tradition (*comprehensive*). Justification, then, is a matter of citing these principles and using them to constrain or derive consequences. Presumably, the broader or more abstract the principle(s), the better founded the justification and the more likely to obtain (or be worthy of obtaining) agreement among rational individuals. Consensual (political) principles of this sort replace the now absent philosophical (metaphysical) principles in justificatory arguments.

If, on the other hand, we look at consensus and justification as being embedded or situated in particular institutions or contexts, using the schools as an example, a different image emerges. Instead of finding a "thin" consensus in which a few principles form a political charter, we find a "thick" consensus involving several strands of virtue, both moral and intellectual, reflecting a range of values.[11] These are less "theorems" than multiple categories and principles for describing, evaluating, and justifying character and behavior. They are expressed and made meaningful in particular institutions and practices, and their interpretation changes over time. Though there may be some presumptive priorities among these values and virtues, they are not easily or strictly ordered or reducible to a few primary principles. Indeed, they are often in conflict with one another. In the ordinary case, the tensions between and among several values and virtues are not resolved by appeal to first principles. Consensual values function less as principles guiding practice (as in Rawls) than as background assumptions for what Schon (1983) calls "reflective practice," a kind of "thinking in action" spurred by immediate problems. For example, dealing with practical matters and problems in a school seldom involves reference to first principles. The organizational and cultural setting of the school and the legitimacy of its existence and purposes are generally taken for granted. Setting a disciplinary policy or providing a health curriculum, for example, will not involve reasoning from some fundamental principles; rather, it will typically focus on specific values (cooperation, self-control) that the schools seek to promote and the most effective ways to achieve them. This does not mean that school policies and practices are beyond question. There is an element of consensus involving intellectual virtue(s) that suggests the desirability of interpreting and questioning consensual values. Nevertheless, there typically is—and perhaps must be—much taken for granted, and questions raised are usually contextual rather than fundamental.

Similarly, ordinary justifications often—perhaps typically—do not involve much theoretical elaboration. Unlike more theoretical justifications, ordinary (political?) justification begins, and often ends, with a form of contextual justification (Herzog 1985, chap. 5). We justify our values and commitments in the first instance by appeals to some widely held value or virtue (e.g., truthfulness) or the rules within some institutional setting or practice; for example, the rules of a school may be invoked to require or prohibit something. Often, that is all the justification that is needed or sought, and at most what is at stake is the invoking or interpretation of the relevant value or rule. Of course, a request for interpretation of, or challenge to, the rules may lead to the broader consideration of consensual values or institutional purposes. The discussion (or, perhaps, political conflict) may then turn to still broader (higher) principles that presumably cut across specific institutions and practices. Conflict at this level may eventually involve the clash of more comprehensive perspectives and the use of political procedures to adjudicate the conflict (e.g., court cases that challenge the authority of school officials vis-à-vis students).

Given these various levels of argument, consensus on what counts as an adequate justification may often be reached at a relatively low level. A call for further justification may or may not be appropriate; in a setting such as the schools, as well as in liberal society in general, certain virtues and habits are likewise shared and taken for granted. More important, these justifications, made locally or contextually, against the background of an institution or practice, apparently meet the test of being "political, not metaphysical." At any step up the justificatory ladder, consensus that a policy or practice is justified may be achieved among individuals or groups. They may agree for different (metaphysical or political) reasons. These are only inadequate as justifications if we add formal or theoretical requirements that they must be subsumed under broader principles of a certain sort. As a practical matter, the search for a broader, more theoretical justification may not lessen disagreement or enhance the consensual pull of the argument. In fact, we sometimes decide that our values or principles have to be modified better to conform to our intuitions about practices or particular cases. Often, as the New York State Education Department curricular guidelines suggest, the result is that (consensual) political processes settle or adjudicate what cannot be resolved by rational appeals to (higher) values or principles; again, this may even change our principles or understandings on the basis of the case at hand. Once we abandon the theoretical expectation of justification as invoking primary principles, there may be several styles of justification, including some that are more contextual and local[12] and some that are simply the result of political processes.

There may be empirical and conceptual objections to this characterization of consensus and justification, whether in the schools or in liberal society generally. First, it might be objected that this benign view of consensus and virtue in the public schools is misleading or simply wrong. Schools vary widely, and the numerous dysfunctional schools can hardly be said to be promoting liberal virtues. Even schools that function well are not suitable models for locating, understanding, and promoting liberal democratic values, because they are not primarily arenas in which equals discuss and work out shared understandings. Rather, the emphasis is on controlling students, making them conform to peer or teacher opinions, and encouraging competition for grades. Versions of this critique arise from many sources. Conservatives decry a lack of a common cultural background produced by segregated and "dumbed down" curricula (Bennett 1988). Liberals and radicals note class differences within the schools and call for the schools to provide genuine equal opportunity or for changes in the larger system that the schools serve (Bowles and Gintis 1976; Persell 1977). In any case, this kind of critique suggests that there is a severe gap between rhetoric and reality in the schools, either as a simple matter of fact or as an intentional rationalization of inequity.

It is important not to oversell the role of the schools in embodying and encouraging liberal democratic values. They are a significant arena—but not the only one—in which moral and intellectual virtues may be developed and practiced in a liberal society (Walzer 1983, 197–99). Moreover, because they involve children, it is doubtful that they could function without some "undemocratic" features involving hierarchy and control, particularly for younger children. This does not, however, preclude schools from helping students, particularly older students, understand moral and intellectual virtues widely agreed upon in our society, especially as the two interact with one another. The schools remain an important institution for "conscious social reproduction" (Gutmann 1988, 46). Keeping in mind the difference between rhetorical consensus and actual practices, the question then becomes, What are the schools reproducing? It is certainly the case, as the ongoing criticisms and pleas for reform illustrate, that the schools do not do enough to promote these virtues, that there is a gap between rhetoric and reality. However, it should also be noted that these criticisms implicitly invoke consensual notions about the schools' role in promoting certain kinds of virtue. If the schools do emphasize control, conformity, competition and so on, they do not announce this as their mission. They would be hard put to announce that their primary moral purposes were to control and segregate students and discourage intellectual questioning. Thus, one can acknowledge some gap between rhetoric and reality without denying the value of seeking a rhetorical consensus that has some basis in reality, for example, in many ordinary practices and in so-called effective schools; these instances serve as models or concrete illustrations of our consensual notions.[13]

A second, more theoretical objection might be that this view of (thick) consensus and (contextual) justification risks incoherence. Making consensual values consistent or justifying them at all, it might be argued, requires the use of some more fundamental principles, otherwise we cannot develop coherent—let alone justified—policies. However, though some broader principles and values have some presumptive priority over others, theoretical coherence is not necessarily obtained by appeal to higher principles. These, too, are subject to dispute and conflicting interpretation; and debates may merely be shifted to a more abstract plane. Tension between and among various values—and strands of virtue generally—is only a sign of incoherence if we expect that all our commitments must somehow be comprehensively ordered and interpreted so that they do not conflict. But this (metaphysical) expectation runs counter to our everyday (political) experience of tension among a number of values as a normal state of affairs (Stone 1988). Higher or more general principles may be rhetorically invoked to help resolve these problems and may have a heuristic value in debate and discussion; but they are also subject to conflicting understandings and inferences. Again, instead of charter principles structuring or resolving conflict, the particulars of any occasion for debate may force the rethinking of these more general principles.

Coherence in a thick consensus, then, is a "reflective equilibrium," a balance between consensual principles of varying levels of generality and more specific or local practices and values, rather than a comprehensive arraying of values and practices under a few charter principles.[14]

There is perhaps a more pragmatic reason for this understanding of consensus and justification in a liberal society. Rawls seeks consensus on fundamental principles as a way of providing protection against the arbitrary use of state power. But it is not clear that a thin consensus, one based upon a few governing principles, will better serve this purpose than consensual agreements that are more numerous, local, and specific to particular institutions. If citizens regard themselves as sharing only commitments such as those Rawls offers and find themselves routinely offering justifications in such basic terms, they might be more—not less—likely to invoke state power to enforce their views of what these commitments prohibit and require. Different and conflicting justifications in terms of these "theorems" become, almost by definition, fundamental disagreements for which state power would seem a logical recourse. Conflict and competing justifications within a thick consensus will tend to be partial, local, and contextual—and thus less likely to degenerate into all-out conflict. The tie that binds a liberal community is likely to be stronger if it does not rely on one or a few (thin) threads but many (thick) interlocking strands of consensus.

It might also be objected that political justifications at a lower, or contextual, level are not really justifications at all or are parasitic on a more complete or theoretical notion of justification, such as that implied by Rawls. That political processes settle issues when there is persistent disagreement does not mean that such settlements are rationally justified. These objections suggest that the adequacy of political justification is to be primarily determined by philosophical or theoretical criteria or (in the current phraseology) that philosophy takes priority over democracy (Bernstein 1987; Rorty 1987). But this becomes problematic once political consensus within a community—for pragmatic purposes and on the basis of differing reasons—replaces some other means (foundation) for determining justificatory adequacy. A consensus on some institutional practice or policy (e.g., health curricula) is not obviously less justified if some higher consensual principle(s) cannot be introduced to support it; relevant, broader philosophical arguments may be less likely to obtain or maintain consensus. Similarly, if resort to agreed-upon political procedures leads to an accepted settlement of a disagreement about a policy or practice (e.g., a court decision about student rights), further philosophical appeals are not likely to create consensus, since philosophical disagreement was probably at the root of the conflict in the first place.

Contextual or political justifications of this sort are not wholly independent of philosophical considerations (even though they may occasionally seem so). Not any reason can be offered for a policy or practice; values and principles cannot be interpreted in infinitely elastic ways. An important aspect of intellectual virtue in a liberal society is the ability to critically evaluate interpretations of values and policies. If philosophy does not take priority over democracy, the latter need not totally forgo some of the advantages of the former. Still, it can be argued that what will count as adequate justification (the scope and structure, and even the substance, of reasons or principles involved) within an institution or community is not determined solely, or perhaps even primarily, by philosophical means. Once justificatory principles, such as those offered by Rawls, are treated as political and therefore grounded in (overlapping) consensus rather than philosophical argument, the issue of justificatory adequacy also becomes a political question.[15] Similarly, in these cases justifications are not likely to be offered in terms of lexically ordered principles. Rather, there are several principles that give conflicting reasons and guidance for actions and policies, and there is no specifiable method or priority rules for resolving these conflicts. For example, the list of virtues cited by Bennett and others—or any comparable list of moral and intellectual virtues—provides reasons for practices or policies. However, they are not arrayed in some precise ordering, and conflict among them is to be expected. Once the community has been made a justificatory court of appeal—through consensus or, failing consensus, political processes—it is difficult to establish philosophical constraints of the sort implied by Rawls. An adequate political or contextual justification may not be simply a condensed version of a fully adequate theoretical argument, even supposing such arguments are available.[16]

The difficulty in seeking justification through a hierarchy of moral principles may be illustrated by Kohlberg's approach to moral education. Kohlberg offers a programmatic means for charting moral development through three levels, each of which includes two stages. The earliest, *preconventional* stages are characterized by compliance based on authority and self-interest, while the middle, *conventional* level involves judgments in response to the opinions and expectations of others or the rules or customs of some larger reference group. At these levels, moral standards typically come from some external source. At the highest, *postconventional* level, moral reasoning is based on internalized, self-chosen, universal principles of reciprocity, justice, and equality (Kohlberg 1981). As Gutmann puts it, "moral autonomy means doing what is right and good *because* it is right and good and not because teachers or any other authorities demand it" (1988, 62). Not surprisingly, Kohlberg invokes Rawls's principles as a model of the "postconventional" form of reasoning.

Leaving aside the question of whether there is a sound empirical basis for Kohlberg's claims, there is considerable room to criticize them as giving undue moral priority to certain kinds of reasoning or principles (Simpson 1989, chap. 1). One of the most telling critiques of this approach is Gilligan's (1982) criticism of its "male" bias. Kohlberg's hierarchy places impersonal, formal, and legalistic analyses of moral positions above a more "female" orientation toward cooperative and personalized solutions of moral dilemmas; it places the "morality of principles," or an "ethic of justice," above "the morality of associations," or an "ethic of caring" (see Gutmann, 1988, 59–64; Rawls, 1971, 462–79). Gilligan's criticism may be recast in terms of contextual justifications and a plurality of primary moral claims. The "different voice" to which she appeals does not seek the resolution of moral conflicts through philosophical appeals to theoretically structured principles. It takes seriously more local and contextual considerations, as well as the importance of interaction in resolving conflict. Similarly, Gilligan's position allows the possibility of plurality of primary moral claims, including those rooted in a "morality of associations." It is thus doubtful that Kohlberg's highest stage of moral reasoning reflects consensual positions either about the structure or substance of moral reasoning or else it can reflect them by overriding other moral commitments that have claims to legitimacy, such as those raised by Gilligan. The expectation that moral reasoning must, like a good theory, be based on a few hierarchically ordered principles may not be an attainable or appropriate aspiration—particularly for guiding a program in moral education.

Improving moral education in the schools, then, may be less a matter of introducing a catechism of core values or a theoretically guided program such as Kohlberg's than of self-consciously doing better what is already being done or, at least, what is (rhetorically) agreed ought to be done. Schools must promote students' physical well-being and provide a safe and orderly environment for learning with reasonable rules fairly enforced. Ordinary procedures and classroom practices should be used to encourage cooperative and other-regarding behavior; much of the hidden curriculum needs to be scrutinized to avoid sending many of the wrong messages noted by critics. The academic curriculum can present more of an intellectual challenge, particularly on moral issues. Finally, the schools can develop more relationships with the community and encourage civic involvement through service requirements or curricula that address local problems. This list is not merely rhetorical or symbolic. Any or all of these tasks can, and are, being pursued in concrete ways. For example, the Child Development Project is revising the hidden curriculum by changing classroom relationships. New York's proposed curricular guides and other programs in critical thinking focus on the improvement of intellectual virtues. There are also any number of citizenship programs in secondary schools that urge students to become involved in community affairs. Finally, there are broader kinds of programs such as Comer's (1988) community-based model of schooling that try to bring together many of these elements, including (perhaps beginning with) assisting families with meeting the basic physical and emotional needs of students. Although these projects and programs do not have the sweeping quality of the reformers' pronouncements about moral education, they are probably a more practical means of translating the elements of rhetorical consensus into reality. This is probably a better strategy—and one more in keeping with consensual principles—than a more theoretical approach; rather than listing values as if they were intellectual principles to guide action, these projects act upon shared values in an institutional setting.

In sum, the argument here is not that democracy takes priority over philosophy, that contextual justifications supplant foundational ones, or that the morality of associations dominates the morality of principles. Rather, the point is that Rawls's implicit views on consensus and justification prejudge these oppositions, favoring the latter in each pair. If we take seriously Rawls's political turn, using moral education as an example, there is a case for reversing Rawls's implicit priorities or at least for seeking a better balance of these competing considerations. Consensus embodied in institutions such as schools can be maintained without some deeper consensus on charter principles. Philosophical arguments may not typically resolve disagreement or override democratic political processes. Contextual justifications emphasizing the preponderance of good reasons in the face of competing principles is more typical and is more likely to obtain consensus than foundational arguments, Rawlsian or otherwise. The pragmatic desire for stability and the restraint of state power is probably better served by *not* seeking a comprehensive theoretical response to the "fact of pluralism." Moral education in the schools may be better advanced through concrete local efforts than through broader programs and pronouncements. Thus, instead of viewing consensus and justification (and reform) from a top-down perspective that emphasizes theoretical structure and lexically ordered, abstract principles, there is much to be said for an alternative, bottom-up perspective involving a plurality of moral principles, contextual considerations and political processes, and actual practices and institutions.

Communitarianism and the Tasks of Political Theory

It may seem as if this analysis amounts to an endorsement of the communitarian critique of liberalism. To be sure, the communitarian critique of liberalism is useful in that it draws our attention to the "situated" character of understandings of values and virtues, for example, in particular institutions such as schools. These, in turn, suggest some things about the nature of consensus and justification in a liberal society. However, this implies the rejection not of liberalism, but rather of some of the expectations expressed in liberal theory. In an odd way the communitarian critique takes liberal political theory too seriously. It reads into liberal societies the individualism expressed in theory when in fact there are probably more ties embodied in the institutions of liberal society (e.g., as reflected in the schools) than the critique acknowledges. It is almost as if the communitarians had disregarded their own injunction to look at actual historical and situated understandings and looked at liberal societies through the somewhat distorting lens of liberal political theory. Moreover, the communitarians' insistence upon the dramatic transformation of liberal individualism probably demands both too much and too little. Given what we have seen in one representative institution of liberal society—the schools—we cannot realistically expect dramatic shifts toward communitarian ideals. Moral education in the schools reflects Hobbesian values and current public consensus about individual responsibilities and duties—neither of which would be easily and significantly shifted in a communitarian direction. Self-conscious programs in moral education might promote such shifts, and the schools can certainly do more to encourage public-regarding behavior. Still, the extent to which the schools can do this is uncertain, given the reluctance of school officials to move beyond obvious consensual values and the potential political difficulties of doing so. At the same time, the communitarian critique takes too little note of some of the positive ways in which the schools and other institutions already promote a variety of virtues, as well as their (perhaps modest) potential for doing more; that is, the critique gives too little credit to ideals of virtue expressed in the institutions of liberal society.

Given this account, the task of theory in a liberal society might be to catalog the variety of virtues, the organizational settings in which these are promoted (or compromised), and the tensions between reasonable expressions of virtue. The political thrust of Rawls's recent work would be more self-consciously anthropological, portraying the values of the community as they are understood and practiced by its members. This approach certainly takes the voices and practices of the community seriously and militates against the temptation of theory to conquer politics (Barber 1988); it also brings theorizing and issues of policy closer together. At the same time, consensus in a liberal society extends to the possibility that individuals will interpret—and even question—basic values in different ways, leading to the fact of pluralism so concisely described by Rawls. However, the solution to the fact of pluralism is not solely, or even primarily, found in an appeal to some political charter but the continuous discussion and interpretation of shared values embodied in institutions. This process of dialogue presumes a shared commitment to intellectual virtues that are also basic, consensual values in liberal societies. It also projects tasks for political theory that go beyond narrowly anthropological analysis of aspects of consensus. The interpretation and questioning of shared values can go beyond direct engagement in some institutional setting; it also generates philosophical speculation about political values and institutions. Consensual values related to intellectual virtue thus create an impulse to pursue more comprehensive and even metaphysical accounts for shared values and practices. These lead to philosophical aspirations for coherence and, ultimately, truth. These aspirations, though perhaps never fully realizable, create a desirable tension and dialogue between philosophy and democracy, a dialogue fully consistent with the consensual values of liberal society.

Notes

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1. The hymn is said to have been written after Fawcett, having accepted a position in a wealthier parish, changed his mind "when love and tears prevailed" and stayed with the poor but close-knit community (Julian 1957, 148).

2. For terminological convenience, the terms *liberal theory* and *liberalism* will be understood here to include liberal democratic theory. The issue here is less the actual or proper relationship of liberalism to democracy than consideration of a strategy for defining and defending either one by reference to shared values.

3. It is important to note here that Rawls explicitly rejects philosophical skepticism. Because of his views on the task of political theory, he seeks to remove basic or "comprehensive" philosophical issues from the discussion, "to apply the principle of toleration to philosophy itself" (1987, 13). Because of the fact of pluralism, we are asked by Rawls not to demand that his political conception be justified by reference to some general moral theory or be seen as "simply another case to which that general moral conception is applied" (1985, 225).

4. This poses a problem for the communitarian critique as well. If the task of theory becomes to reach an agreement on consensual principles that reflect the community's self-understandings, liberals may claim, with some justification, that their views better reflect the community's ideals than those (vaguely) offered by communitarians. The communitarian position thus faces a tension between a historicist epistemology and normative ideals of public- or other-regarding behavior. The former suggests that a liberal society's governing principles and self-understandings are established by the community, while the latter implies a rejection of those in favor of some more communitarian alternative. By the same token, though liberal principles may better reflect liberal society's self-understandings than those of the communitarians, the views of the community may be some distance from those of many prominent liberals such as Rawls. Finally, it seems that the relevant "community" to which Rawls is addressing himself is the community of liberal political theorists from Hobbes to the present. This may be a worthy enterprise, but the relationship of this community to wider public self-understandings is problematic.

5. A number of contemporary liberals have sought to provide an analysis of virtue in liberal society (Galston 1982, 1988; Raz 1986; Shklar 1984). My analysis is consistent with their approach, though it offers a more "microscopic," or institutional, analysis.

6. It might be wondered why the consideration of moral education does not take as its point of departure some general, theoretical treatment of educational issues such as Gutmann's (1988) or a more specific approach such as Kohlberg's. The reason for beginning the discussion in this way is to follow the logic of Rawls's appeal to overlapping consensus, a strategy that has a parallel in recent political rhetoric concerning moral education. This approach is not theoretical, at least not as Gutmann's is, and it leads to some skepticism about Kohlberg's program.

7. For example: "A healthy lifestyle is more than just eliminating smoking and drinking from our habits. Rather, it is a way of life. It can decrease significantly the risk of disease, while increasing substantially the chances of living healthfully into the eighth decade of life. It is predicated on the idea that our chances of self-fulfillment are increased or decreased directly by our level of wellness. A healthy lifestyle includes almost every decision we make—from when we get up in the morning to when we go to bed; it involves eating, drinking, driving, shopping, socializing, exercising, cleaning, working, loving" (Seffrin and Torabi 1984, 12).

8. For example, Liebert, in a critique of Kohlberg's approach, notes a gap between cognition and behavior. He cites an experiment in which theological students on the way to give a lecture on the Good Samaritan were confronted with someone in distress: "Most of the seminary students did *not* help the victim in this situation, and neither the topic on which they were to lecture nor the value they placed on religious commitment was associated with helping" (1984, 187). The same gap might occur in the program envisioned by Bennett and others.

9. The term *intellectual virtue* here is concerned with *phronesis*, or practical wisdom, rather than with the more abstract pursuit of knowledge for its own sake (dianoetic virtue).

10. Sichel discusses some of the criticisms of Kohlberg's theory as wrongly separating form and content, judgment and behavior (1988, 193–99). She also provides a useful list of "suitable intellectual vir-

tues" that may help bridge these gaps: "the ability to discern what the problem is; analytical abilities to discern what moral excellences are involved with the various dimensions of the problem and with alternative solutions of the problem; . . . data collecting skills to choose which information and data relate to the problem and . . . to the agent's moral excellences; imagination and creativity to envisage the problem in a different way . . . to forsee the success or failure of various solutions . . . and how these solutions might affect the agent's moral excellences; reasoning ability to discern and evaluate . . . presently held moral excellences and then possible implications for moral judgment and action" (260–61).

11. The notion of a thick consensus is analogous to Geertz's (1973) notion of thick descriptions of culture. It is also offered in contrast to Rawls's notion of the priority of a thin theory of the good (1971, 395–99); rather, what is implied here is a thick theory of consensual goods.

12. For example, Simpson, arguing against a "rationalist" approach such as Kohlberg's, notes, "If gentleness, humility, generosity, and kindness are considered moral qualities, then the rationalist [Kohlberg's] account faces a more severe complaint than narrow and arbitrary classification [of moral terms]. These virtues are not only approved without reference to principles but are also foreign to a principled approach. . . . Virtue cannot be generally represented as conscientious adherence to principle. The norms typical of many virtues require the use of judgement in order for us to know how to respond appropriately to the particularity of a situation not adequately governed by rules" (1989, 10). See also Stout 1986, 34–38.

13. There is a limiting case here: if the gap between rhetoric and reality is intentional and manipulative (e.g., to rationalize class reproduction), no appeal to political consensus, Rawlsian or otherwise, will suffice to bridge the gap; for there is no consensual community.

14. The notion of "reflective equilibrium" used here is not the same as in Rawls, but he describes the idea very nicely: "The notion varies depending upon whether one is to be presented with only those descriptions which more or less match one's existing judgments except for minor discrepancies, or whether one is to be presented with all possible descriptions to which one might plausibly conform one's judgments together with all relevant philosophical arguments for them. In the first case we would be describing a person's sense of justice more or less as it is although allowing for a smoothing out of certain irregularities; in the second case a person's sense of justice may or may not undergo a radical shift" (1971, 49). Rawls sees the second case as the concern of moral philosophy; the implication here is that the first ("smoothing out irregularities") is a plausible understanding, once principles are seen as political (consensual) and not metaphysical (philosophical).

15. As Barber puts it, "Where knowing becomes doing, public opinion agreed upon overrules private 'truth' disputed. The philosopher may trump the drop out in private; but the citizen, even when he is a drop out, trumps the philosopher in public" (1988, 15). Similarly, Walzer argues that "philosophical validation and political authorization are two entirely different things. They belong to two entirely different spheres of human activity. Authorization is the work of citizens governing themselves among themselves. Validation is the work of the philosopher reasoning alone in a world he inhabits alone or fills with the products of his own speculations" (1981, 397).

16. In defense of contextual justifications, Herzog notes that "nothing is certain, nothing is fixed in such an argument. We have no incentive to find immutable premises, no reason to devote much energy to epistemology, theology, metaphysics, and the like. We cannot plausibly claim that our conclusions should be applied in all times and places. A contextual justification may then seem much weaker than a foundational one. But successful foundational justifications are philosophers' pet unicorns; we have yet to see one. Contextual justifications provide a preponderance of good reasons, so they are good enough to qualify as justifications even if they don't deliver the certainty foundational arguments might" (1985, 225).

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RESEARCH NOTE

MEASURING VALUE CHANGE IN WESTERN INDUSTRIALIZED SOCIETIES: THE IMPACT OF UNEMPLOYMENT

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During the past two decades a four-item battery administered in biannual Euro-Barometer surveys has been used to measure changing value priorities in Western European countries. We provide evidence that the measure is seriously flawed. Pooled cross-sectional time series analyses for the 1976–86 period reveal that the Euro-Barometer postmaterialist–materialist value index and two of its components are very sensitive to short-term changes in economic conditions, and that the failure to include a statement about unemployment in the four-item values battery accounts for much of the apparent growth of postmaterialist values in several countries after 1980. The aggregate-level findings are buttressed by analyses of panel data from three countries.

The topic of value change in advanced industrial societies has generated an ongoing, multifaceted controversy. One important aspect of this controversy centers on methodological aspects of studies of value change and, more particularly, on the measures of value priorities in the widely used Euro-Barometer surveys.[1] We join this methodological debate. Ronald Inglehart has argued that aggregate- and individual-level analyses demonstrate the adequacy of these measures. We argue that this claim is unwarranted. Reanalyses of Euro-Barometer data show that the four-item value battery is very sensitive to changes in unemployment rates, and reanalyses of the *Political Action* panel data reveal substantial instability in the factors measured by the battery.[2]

The point at issue is the extent to which short-term shifts in the economy affect responses to the four-item materialist-postmaterialist index. If Inglehart's *socialization hypothesis* is correct, value priorities in Western publics are largely products of economic conditions that prevailed in the past, i.e., during people's preadult years (e.g., Inglehart 1990a, 56, 68). Although he also offers a *scarcity hypothesis*, which acknowledges the presence of period effects, he argues that these are minor, and that values in adult populations exhibit high levels of stability. Thus, they do not vary markedly in response to current economic circumstances. Moreover, in keeping with the scarcity hypothesis, the direction of period effects is such that decreases in material well-being are associated with decreases in the proportion of postmaterialists and increases in the proportion of materialists.

We show that the four-item values measure, which has been included in Euro-Barometer surveys for nearly twenty years and which forms the basis for much of the work on value change in advanced Western societies, does not behave in accordance with these expecta-

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tions. The measure manifests different over-time trends in various European countries, and it reacts strongly to short-term movements in the macroeconomy. Moreover, some of the relationships involving economic variables are contrary to the scarcity hypothesis.

These aggregate-level findings are important but they constitute only part of our evidence. Inglehart also argues (e.g., 1990a, chap. 3) that values as measured by the Euro-Barometer items are highly stable at the individual-level. This argument is buttressed by covariance structure analyses showing strong stability in the underlying value factors that the indicators are designed to measure. In response, we argue that these analyses are problematic and may seriously overestimate individual-level stability in two of the three countries for which requisite data are available.

Our analysis proceeds in three steps. First, we examine trends in the percentages of recorded materialists and postmaterialists in eight European countries over the 1976–86 period. Second, we analyze the impact of changing economic conditions on the proportions of materialists and postmaterialists as well as the proportions choosing specific value priorities in the eight countries over this period. Third, we use the *Political Action* panel data to reexamine the extent of individual-level instability in the value items and the underlying factors that they are hypothesized to measure.

Methods and Measures

The four-item measure of materialist-postmaterialist value priorities requires respondents to designate which one of the following they consider personally most important, and which one they consider second-most important: (a) maintain order in the nation; (b) give people more say in the decisions of the government; (c) fight rising prices; (d) protect freedom of speech. Here, we employ data gathered using this measure for the 1976–86 period in eight countries: Belgium, Denmark, France, Great Britain, Ireland, Italy, the Netherlands, and West Germany.[3] The economic series used includes real gross domestic product (GDP) per capita (year-to-year percentage change); unemployment (the percentage of the total labor force unemployed); and inflation (the year-to-year percentage change in the consumer price index) (OECD 1988, 39, 44, 83). The GLS-ARMA variant of pooled cross-sectional time series analysis (Stimson 1985) is employed to estimate the impact of these macroeconomic variables on value priorities as measured by the four-item battery. To analyze individual-level stability in value priorities we rely on panel data gathered in national surveys conducted in the Netherlands (1974–1979), West Germany (1974–1980), and the United States (1974–1981). Both waves of these panels include Inglehart's four- and eight-item measures of value priorities, and the data are those used to support his argument that values manifest high levels of individual-level stability. We first determine the amount of change in responses to the individual items and the summary materialist-postmaterialist index. Then, we conduct confirmatory factor analyses (Long 1983) to assess the stability of the hypothesized underlying value constructs.

Findings

Country Trends

In Inglehart's most recent studies (1990a, 1990b) much of the evidence for a *general* value shift toward postmaterialism rests on analyses of the four-item measure using aggregated Euro-Barometer surveys for several European nations. The merging of the national-level data is justified as a way to decrease sampling error in age-cohort analyses. However, aggregating the data in this way may obfuscate intercountry differences in trends that would be apparent if each country were examined separately (Boeltken and Jagodzinski 1985, 463–64). Also, if there are cross-national differences in economic trends, this procedure may mute the impact of economic variables in analyses of the dynamics of responses to the value measure.

Graphing the recorded percentages of materialists and postmaterialists for the 1976–86 period shows quite different patterns for the eight countries. In some cases (Denmark, Great Britain, the Netherlands, and West Germany), there are clear overall trends indicating decreases in the percentage of materialists and increases in the percentage of postmaterialists (see Figure 1). However, there are equally as many cases (Belgium, France, Ireland, Italy) where these trends either are very weak or completely absent; and in the Belgian, French, and Italian cases, the percentage of postmaterialists actually is smaller in 1986 than it was a decade earlier (see Figure 2).

One may formalize these impressions by regressing the percentages of postmaterialists and materialists on time in each country over the eleven-year period. In the British, Danish, Dutch, and West German cases, there are significant positive trends in the proportion of postmaterialists, and in these cases as well as the Irish, there are significant negative trends in the proportion of materialists (Table 1). The other countries are different—the trend coefficients for postmaterialists are negative (but insignificant) in Belgium, France, and Italy. Also, while trends in the percentages of materialists are negative for France and Italy, the coefficients are insignificant; and for Belgium, the coefficient is actually positive. Moreover, in several cases there are large deviations from the trend lines (Figures 1 and 2), and substantial variance is unexplained (Table 1). The latter findings suggest that short-term factors influence the percentages of responses classified as materialists and postmaterialists in a given country in a particular year.

Table 1. Linear Trend Models of the Percentages of Postmaterialists and Materialists, 1976–86

	Post-materialists		Materialists	
Country	*b*	r^2	*b*	r^2
Belgium	−0.26	.21	0.72	.26
Denmark	0.75*	.66	−1.78*	.77
France	−0.13	.05	−0.35	.10
Great Britain	0.92*	.71	−1.66*	.64
Ireland	0.24	.31	−0.75*	.43
Italy	−0.16	.07	−.086	.15
Netherlands	0.73*	.41	−1.60*	.63
West Germany	1.34*	.72	−2.50*	.77

*$p \leq .05$, two-tailed test.

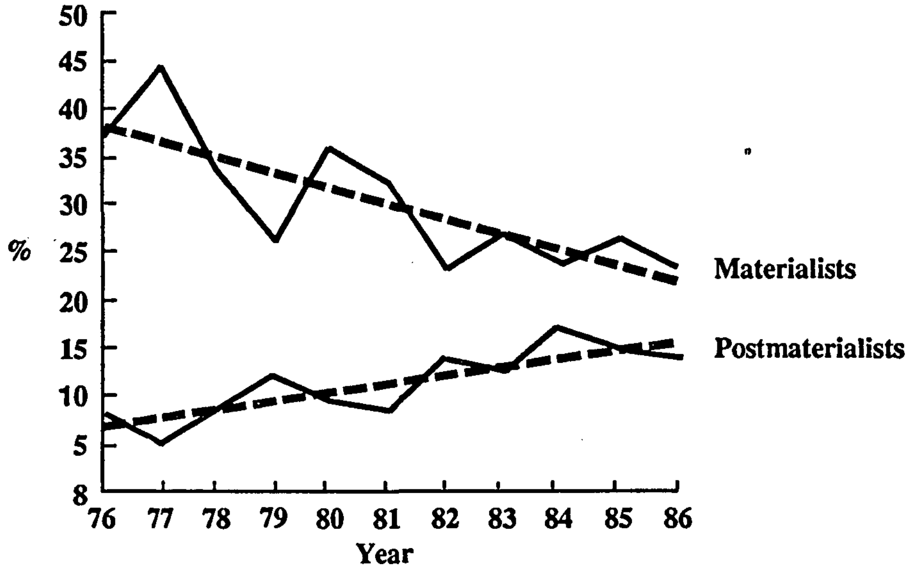
Value Priorities and the Macroeconomy

We first consider the influence of changes in economic conditions on the percentages of materialists and postmaterialists as measured by the four-item battery. If the battery adequately measures value priorities largely shaped by early life socialization processes, then economic conditions should not have significant contemporaneous effects on either the proportions of materialists and postmaterialists, or the priority assigned to individual items in the measure. However, if the measure is flawed or Inglehart's theory of value stability is incorrect, then such effects might be anticipated.

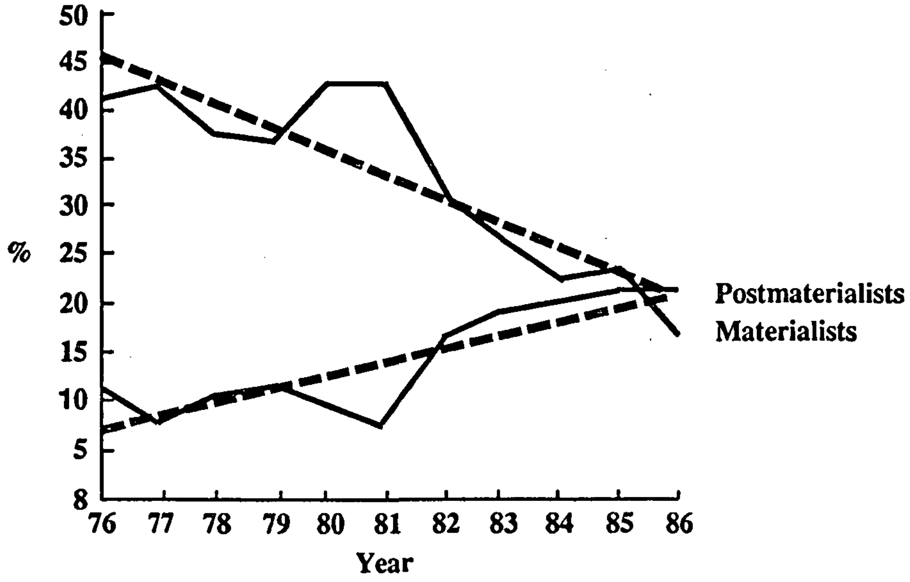
As noted above, Inglehart (1990a, 77–81) does not argue that values are totally impervious to short-term period effects such as those that might be engendered by changes in the macroeconomy, and he displays graphs for the 1970s and

Figure 1. Trends in the Percentages of Materialists and Postmaterialists in Great Britain and West Germany, 1976–1986

A. Great Britain



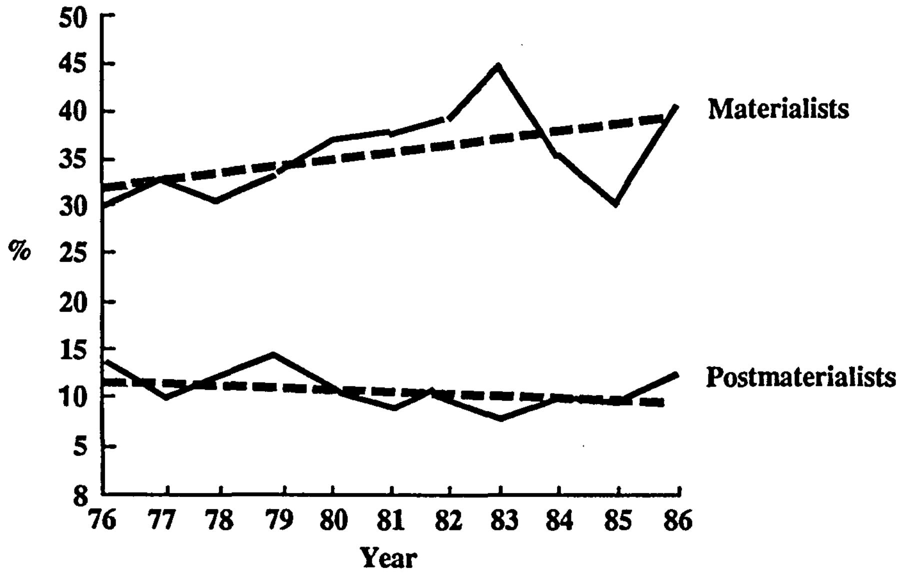
B. West Germany



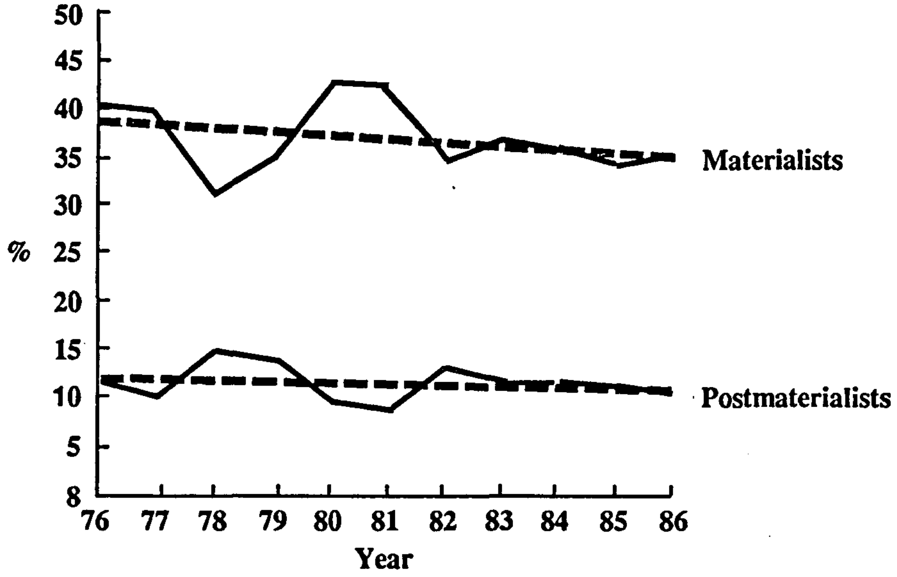
Note: Dashed lines are fitted linear trends.

Figure 2. Trends in the Percentages of Materialists and Postmaterialists in Belgium and France, 1976–1986

A. Belgium



B. France



Note: Dashed lines are fitted linear trends.

1980s suggesting the presence of such effects. However, this analysis does not enable one to determine to what extent the observed period effects are a result of limited value instability as opposed to being an artifact of measurement problems associated with the index. If the four-item measure taps underlying values, according to his scarcity hypothesis, increasing economic hardship should be associated with decreases in the percentage of postmaterialists and increases in the percentage of materialists.

If the four-item index is an inadequate measure of values, there are other possibilities. Note that one of the items is "fight rising prices." This statement might tap a deep-seated aversion to inflation, but alternatively, responses to it might largely reflect the salience of inflation as a *topical political issue* (Flanagan 1982a, 430–31, 1982b, 113). If this is the case, when inflation subsides the issue agenda will change and many respondents will no longer select the "fighting prices" item as their top "value" priority. In the spirit of Inglehart's (1990a) analysis acknowledging the presence of period effects, one might argue that their magnitudes are crucial for assessing the validity of this "issue-priority" hypothesis. For example, if the impact of inflation on the percentage of respondents selecting the prices item as their first priority is positive but modest in strength, this could be interpreted as being consistent with an argument about limited economic period effects in the inflation-ridden 1970s, these operating within a broader context of secular trends toward postmaterialism.

But, what about unemployment? Despite the fact that high levels of employment long have been considered a sine qua non for ensuring material well-being in Western industrialized societies, the four-item battery does not include a statement regarding the importance of employment. How, then, will someone concerned about unemployment respond? If the battery taps general, deep-seated values and not transitory issue concerns, then a materialist would choose one of the materialist items (fight rising prices, maintain order), and a postmaterialist would choose one of the postmaterialist options (give people more say, protect freedom of speech). In contrast, if people respond to the battery primarily in terms of currently salient political issues, then respondents worried about adverse trends in unemployment, even if they hold materialist values, would not necessarily select a materialist item.

Analyses of data gathered over a time interval characterized by substantial changes in inflation and unemployment are crucial for assessing these alternatives.[4] The 1976–86 period is ideal in this respect. In many Western countries high rates of inflation in the mid- and late-1970s were followed in the early 1980s by a deep recession accompanied by decreases in inflation and sharp increases in joblessness.[5] If the issue–salience hypothesis is correct, in this latter period many persons formerly concerned with inflation abandoned the "fight rising prices" statement in the four-item battery and sought an alternative that reflected their current preoccupation with unemployment. Since there is no such item, which one did they choose as their top priority? We hypothesize that many selected "give people more say in government" to express their preference for policies to alleviate the problem at hand. The probability of this response seems substantial given that in the 1980s a number of Western European countries were governed by newly resurgent conservative parties that stressed the fight against inflation and demonstrated their willingness to sacrifice large numbers of jobs to win that battle. Since the "more say" item is designated as an indicator of postmaterialist values, unemployment rates should be *positively* associated with the percentage categorized as postmaterialists and *negatively* associated with the percentage classified as materialists by the four-item index. Although such a pattern is consistent with the existence of period effects, it clearly contradicts Inglehart's scarcity hypothesis that economic adversity is associated with an increase in the percentage of respondents offering materialist responses to his four-item battery.

Given that various aspects of macroeconomic change are interrelated, it is important to control for the possibly confounding impact of other economic variables when assessing relationships between unemployment and responses to the four-item value battery. Here, we do so by conducting a pooled cross-sectional time series regression analysis with unemployment and inflation rates and annual changes in real GDP per capita as predictor variables. Our model is:

$$
\begin{aligned}
\mathrm{VT}_t = \mathrm{C} &+ \beta_1\mathrm{GDP}_t + \beta_2\mathrm{UNEMP}_t \\
&+ \beta_3\mathrm{INF}_t + \Sigma\beta_{4\text{-}10}\mathrm{CN}_{1\text{-}7} \\
&+ \theta(\mathrm{B}_{t-i})/\phi(\mathrm{B}^{t-i})\mathrm{e}_t
\end{aligned}
$$

where:

C = constant;
VT_t = % of postmaterialists or materialists in year t;
GDP_t = % change in real GDP per capita in year t;
UNEMP_t = % unemployed in year t;
INF_t = % change in consumer price index in year t;
$\mathrm{CN}_{1\text{-}7}$ = country dummies;
e_t = error term.

Diagnostics based on preliminary analyses using the three economic series confirm the existence of heteroscedasticity in the residuals and, accordingly, the analyses are rerun including dummy country variables (Great Britain is the reference category). Residual diagnostics for the second set of analyses indicate the presence of time series dependencies, which are modeled as a first-order autoregressive process with a pooled ϱ estimate.

Analyses of this model (Table 2) show that changes in real GDP per capita have significant ($p < .05$) positive impact on the proportion of postmaterialists relative to the proportion of materialists, a significant positive impact on the absolute percentage of postmaterialists, and a significant negative impact on the absolute percentage of materialists. Increases in the consumer price index have significant negative effects on the relative and absolute percentages of postmaterialists, and significant positive ones on the percentage of materialists. The GDP and inflation relationships are consistent with the existence of economic period effects on responses to the four-item index, and the signs on the coefficients are consistent with the scarcity hypothesis. Unemployment, however, behaves very differently. Increases in the jobless rate have a large *negative* impact on the percentage of materialists, and a *positive* impact on the percentage of postmaterialists, measured both relative to the percentage of materialists and absolutely.[6]

Analyses of the percentages selecting each of the four items in the battery as their top priority show that the impact of GDP per capita is consonant with the scarcity hypothesis. Although not all coefficients are significant, increases in GDP have positive effects on the two designated postmaterialist items (more say in government, protect free speech) and negative ones on the two materialist ones (fight rising prices, maintain order in the nation) (Table 2). Similarly, inflation has a significant positive influence on the percentage selecting prices, and a negative influence on the percentage choosing more say in government. However, unemployment has a large *negative* relationship with "fight rising prices," and large *positive* one with "more say in government." These latter results are exactly what the issue-salience hypothesis predicts given the absence of an unemployment statement in the four-item battery.

Table 2. Pooled Cross-Sectional Time Series Analyses of the Impact of Macroeconomic Variables, 1976–86

Predictor Variables	Post-materialists–Materialists		Post-materialists		Materialists		Fight Prices		More Say		Maintain Order		Free Speech	
	b	*t*	*b*	*t*	*b*	*t*	*b*	*t*	*b*	*t*	*b*	*t*	*b*	*t*
Constant	-30.71	-3.40*	6.60	2.15*	40.06	6.92*	38.63	9.50*	16.97	4.26*	33.61	6.04*	12.96	4.25*
Real GDP per capita[a]	0.93	2.39*	0.40	2.55*	-0.71	-2.57*	-0.27	-1.44	0.27	1.75	-0.59	-2.44*	0.32	2.85*
Unemployment rate[b]	9.85	2.99*	2.34	1.92	-7.85	-3.71*	-8.72	-5.94*	4.75	3.37*	0.27	0.13	0.70	0.64
Consumer price index[c]	-4.63	-3.57*	-1.35	-2.59*	3.31	4.19*	3.46	0.61	-1.75	-3.18*	-0.07	-0.09	-0.68	-1.47
Country														
Belgium	-9.44	-2.12*	-0.91	-0.62	8.60	2.67*	21.98	12.69*	-10.20	-5.22*	-9.20	-3.62*	1.33	0.87
Denmark	-1.88	-0.47	0.96	0.66	2.93	1.08	-10.91	-7.31*	-8.00	-4.09*	17.24	5.17*	2.91	1.49
France	-5.39	-1.52	1.62	1.17	6.96	3.05*	5.89	2.35*	-9.22	-4.30*	0.47	0.14	4.35	2.80*
Ireland	-18.04	-4.79*	-4.97	-3.76*	13.16	5.03*	13.09	6.66*	-3.35	-1.52	-1.54	-0.49	-6.83	-6.53*
Italy	-16.30	-4.07*	-2.31	-1.72	14.09	4.66*	-2.92	-2.04*	-3.36	-1.75	14.14	6.25*	-5.59	-5.14*
Netherlands	6.16	1.63	6.44	3.93*	0.30	0.13	-5.40	-3.15*	-2.23	-1.05	5.80	2.41*	4.90	3.25*
West Germany	-2.28	-0.46	2.33	1.17	4.56	1.50	-3.22	-1.77	-2.60	-1.07	10.48	3.74*	-1.86	-1.34
$\bar{R}^2$	.55		.55		.52		.85		.52		.59		.56	

[a] Year-to-year percentage change.

[b] Percentage of total labor force, natural log transformed.

[c] Year-to-year percentage change, natural log transformed.

*$p \leq .05$, two-tailed test.

Table 3. 1985 Percentages of Postmaterialists and Materialists, and Predicted Percentages at 1980 Unemployment Rates

	Postmaterialists			Materialists		
Country	Actual	Predicted	Difference	Actual	Predicted	Difference
Belgium	9.6	8.8	-0.8	30.2	36.9	+6.7
Denmark	18.2	11.5	-6.7	22.9	30.5	+7.6
France	12.1	10.7	-1.4	35.2	37.5	+2.3
Great Britain	13.9	9.6	-4.3	23.1	30.0	+6.9
Ireland	6.4	4.3	-2.1	40.9	42.6	+1.7
Italy	8.5	7.0	-1.5	29.1	43.9	+14.8
Netherlands	24.3	16.9	-7.4	17.2	27.7	+10.5
West Germany	20.9	11.6	-9.3	23.8	36.2	+12.4
$\bar{X}$ Eight Countries	14.2	10.1	-4.2	27.8	35.7	7.7

How large are the effects associated with changing economic conditions? Since the average percentage of postmaterialists and materialists differs across the eight countries, the bulk of the explanatory power in our model might stem from the country dummies that control for heteroscedasticity in the regressions. If so, period effects associated with short-term changes in the economy would be substantively insignificant. This is not the case. Running the analyses stepwise, entering the country dummies and then the three economic series, reveals that the economic variables account for an additional 30% of the variance in the postmaterialist series, and an additional 36% in the materialist one (data not shown). Similarly, the economic variables enhance the explained variance in the "fight rising prices" and "more say in government" series by 32% and 39%, respectively. In sharp contrast, the increment in the adjusted R-squared for "protect free speech" is much smaller (13%), and is actually negative (−1%) for "maintain order in the nation." These differences in the explanatory power of the economic variables are consonant with the hypothesized sensitivity of the four-item value battery to the changing salience of inflation and unemployment on the issue agenda.

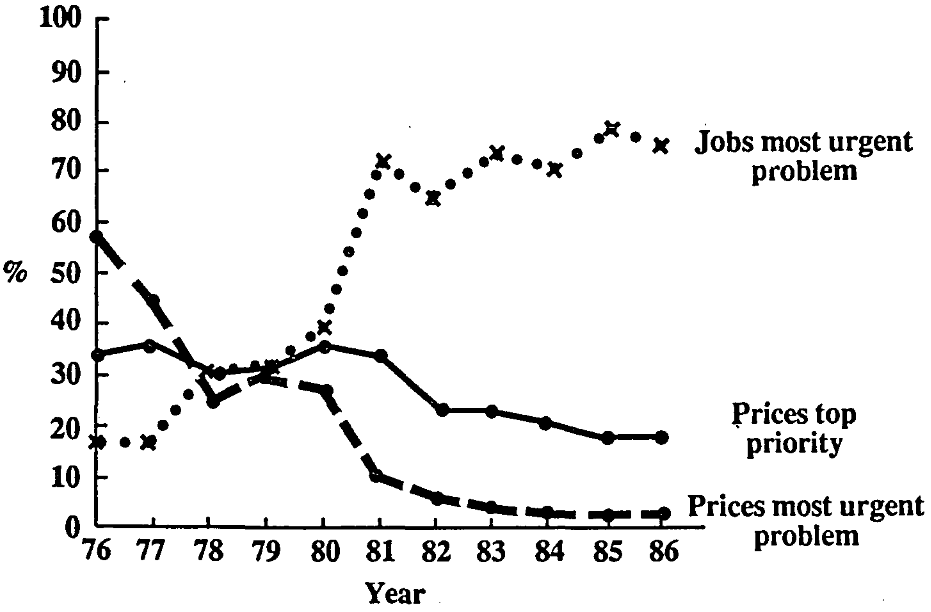
To gauge the impact of changes in unemployment on levels of postmaterialists and materialists measured by the four-item index, we may use our model estimates to predict mid-1980s percentages of postmaterialists and materialists in the eight countries if unemployment had remained at its 1980 level in each of them. In each country the cohort of postmaterialists would have been smaller than that recorded, and the percentage of materialists would have been larger (Table 3). Some differences are sizable, and the largest ones occur in the four countries (Denmark, Great Britain, the Netherlands, West Germany) manifesting the sharpest apparent trends toward postmaterialism after 1980. In the German case, for example, the recorded percentage of postmaterialists was 20.9% in 1985, whereas the predicted value if unemployment had not increased after 1980 is only 11.6%. Moreover, in all four countries the predicted percentage of materialists is substantially greater (by 6.9% to 12.4%) than the recorded figure.

Such differences have implications for conclusions about longer term trends toward postmaterialism in these countries from the mid-1970s onward. The recorded increases between 1976 and 1985 range from 6.2% in Britain to 10.1% in the Netherlands (data not shown). However, the predicted increases under the assumption of stable unemployment (at 1980 levels) range from 0.4% to 2.7%. In Belgium, France, and Italy the 1976 figures already are larger than the recorded 1985 figures, and the 1976-85 increase is trivial (0.3%) for Ireland. However, if unemployment had not increased after 1980, Ireland too would have witnessed a decrease in the percentage of postmaterialists, and the decreases in the other three countries would have been slightly greater than those recorded by the four-item index.

The British Case—A Closer Look

The hypothesis that responses to the four-item index are closely associated with the issue salience of inflation and unemployment may be investigated in greater detail by using independently administered survey questions designed to tap public issue concerns. British Gallup Poll data for the 1976-86 period are well-suited for this purpose. In each of the monthly polls respondents were asked to designate the country's "most urgent problem." Selecting the poll conducted in the same month as the Euro-Barometer surveys we can compare responses to the "problem" question with those to the four-item value battery in the Euro-Barometers. In 1980 the percentage citing unemployment as the most urgent problem increased dramatically and remained at a high level thereafter, and the percentage mentioning inflation fell sharply and subsequently remained very low. The percentage mentioning prices as a top value priority in the British Euro-Barometers also decreased significantly in the early 1980s, this trend very closely tracking the percentage citing prices as an issue (Figure 3). These parallel trends suggest that the unemployment and price issue series are highly correlated with the price value series. This is the case—the correla-

Figure 3. Prices as Top Priority in Euro-Barometer Surveys and Prices and Unemployment as Most Urgent Problems in Gallup Polls, Great Britain, 1976–1986



tions (r) between prices and unemployment as issues in the Gallup and prices as a value in the Euro-Barometer are +.83 and −.81, respectively. Regressing (GLS) the Euro-Barometer price series on the Gallup inflation and unemployment issue series shows that the latter two variables explain 44% and 40%, respectively, of the variance in the former.

In the British case, the decrease in prices and the increase in jobs as the primary national problem coincided with the presence of the first Thatcher Conservative government (1979–83), which made the battle against inflation its top economic policy goal and disavowed responsibility for unemployment. Under the impact of Thatcher's monetarist economic policies, unemployment soared in the early 1980s, and subsequently remained at depression-era levels (Whiteley 1986, chap. 5). Inflation, in contrast, first increased and then subsided. These contrasting macroeconomic trends dramatically altered the political issue agenda, bringing jobs to the forefront of public attention while relegating price increases to obscurity.

As shown, the "fight prices" item in the four-item value battery is strongly correlated with the salience of inflation and unemployment as issue concerns and, we argue, because an employment statement was not included in the value battery, the sharply escalating joblessness of the 1980s produced a marked *decrease* in the percentage of British Euro-Barometer respondents categorized as materialists. Relatedly, recall that rising unemployment is positively associated with a propensity to choose the "more say" item, hence tending to produce an *increase* in the percentage of respondents categorized as postmaterialists. The British case thus illustrates the point that value trends that appear paradoxical in terms of Inglehart's scarcity hypothesis are quite understandable when viewed as a methodological artifact of how people respond to the four-item value battery.

Individual-Level Instability

The aggregate responses to the four-item value measure fluctuate much too abruptly to be accounted for by the onward march of generational replacement as Inglehart's socialization hypothesis would require. Rather, these sizable short-term aggregate movements imply individual-level change. Inglehart and other analysts have argued that evidence regarding individual-level stability in responses to value measures is vital because it directly addresses the thesis that value priorities are largely a product of early life socialization, and that, once acquired, they are resistant to change. Our hypothesis that responses to the Euro-Barometer items are substantially issue-driven requires, however, that they manifest individual-level instability.

Unfortunately, panel data that would enable us to investigate individual-level stability in responses to the four items in each of the eight countries do not exist. However, we do have the panel data for the Netherlands, West Germany, and the United States that Inglehart used to analyze individual-level value stability. One measure of instability is simply the percentage of panelists who change responses over the two waves of interviews. Calculating this figure for the four-item measure shows that response "turnover" is massive: in the Netherlands, 53% change the item selected as most important, and the comparable figures for West Germany and the United States are 60% and 55%, respectively (Table 5). Moreover, change is not confined to movement among items that would leave the respondent categorized as a materialist or a postmaterialist. Overall, 38% of the Dutch, 48% of the German and 47% of the Americans change their value classification across the two interviews.

In his most recent book, Inglehart (1990a, chap. 3) has argued that turnover analyses are inadequate because they fail to consider measurement error in survey responses (see also Dalton 1981; De Graaf 1988, chap. 2). The claim is that at the level of underlying constructs, values exhibit impressive stability. In support of this argument Inglehart conducts covariance structure analyses of the American, Dutch, and German panel data. The model contains three latent constructs: formative security, value priorities at t_1, and value priorities at t_2. The indicator variables for the latter two constructs are indices based on the four-item value battery and an eight-item value battery[7] administered in the two waves of the panel surveys. Thus, there are two indicators of values at time t_1 and two at time t_2. LISREL analyses of value factors show extremely high levels of stability in the Netherlands, but those for the United States and West Germany indicate some instability (Inglehart 1990a, 122–24).

These analyses are problematic for three reasons. First, overall x^2 goodness-of-fit statistics for the models are not reported. Adjusted goodness-of-fit measures (Joreskog and Sorbom 1988, 42) are provided, but these are difficult to interpret since they may have very high values even for models that would be judged to have a poor fit according to x^2 tests and other diagnostics. Second, before the release of LISREL 7, most LISREL analyses were performed using maximum likelihood (ML) procedures. However, ML test statistics assume multivariate normal distributions and interval-level data. With ordinal data such as the value priority indicators, estimation should be performed using a weighted least squares procedure such as that in LISREL 7 (Joreskog and Sorbom 1988, 192). Third, diagrams of the models do not reveal if errors for the same indicators in the two waves of interviewing were permitted to covary (Inglehart 1990a, 120–24). Measurement effects of various kinds typically produce such correlated errors in panel data (Bollen 1989, 235; Long 1983, 29), and failure to model them risks overestimating the stability of underlying factors over time.

The portion of the model dealing with value stability can be analyzed using confirmatory factor analysis procedures. To assess the stability of underlying values, the key parameter is ϕ_{21}, the correlation between the underlying factors at times t_1 and t_2. Without correlated errors for the same indicator in the two waves of interviewing (θ_{31} and θ_{42}), the model has one degree of freedom; however, when these parameters are included it is not identified (Long 1983, 36–55). Here, we address this problem by assuming a range of plausible values for the correlated errors. We first assume zero values, that the errors are unrelated. Next, we test models with nonzero error covariances.

We begin with the Dutch model without correlated error terms.[8] The analysis (Table 4) indicates that the two underlying factors are highly correlated—indeed, the WLS ϕ estimate exceeds 1.0. Specifying correlated errors and fixing their values (at .32 and .36 as suggested by LISREL modification indices) leaves the fit essentially unchanged. However, the correlation between the two factors falls sharply (to .62), thereby suggesting that the t_1 factor can account for less than 40% of its t_2 counterpart. Making a more modest assumption about error covariances (.20), improves the fit dramatically (x^2 = .65, p = .420) and ϕ becomes .84. This coefficient is quite substantial, but again it indicates some over-time volatility—about 30% of the variance in the t_2 factor is unexplained.

There is evidence of greater instability in the West German and United States analyses. In the German case, ϕ = .74 when the θ's are uncorrelated, but when they are correlated ϕ falls to .47 for the best fitting model, leaving nearly 80% of the variance in the t_2 factor unexplained. In the U.S. case, the zero error covariance assumption yields a ϕ = .98, thereby showing high stability. Note, however, that the fit is problematic ($\chi^2 = 42.42$, $p = .000$). Both models specifying correlated errors have much better fits and both produce much lower ϕ values. For the best fitting U.S. model the t_1 factor can explain only about 40% of t_2's variance. Conclusions about over-time stability at the level of the underlying factors thus depend heavily on assumptions made about the error covariances.[9]

Finally, we may use the confirmatory factor models to investigate individual-level relationships between the value factors and answers to a question in the second wave surveys asking respondents their opinion about the importance of a list of current issues and problems.[10] Two of the items are inflation and unemployment. If our hypothesis about how inflation and unemployment issue concerns influence responses to the value items is correct, we would expect that inflation mentions would correlate *negatively* with the value factors, whereas unemployment mentions would correlate *positively* with them. Also, given the presence of "fight rising prices" in the four-item battery and

Table 4. Confirmatory Factor Analyses of Value Priorities, National Panel Surveys: The Netherlands, West Germany, and the United States

	Netherlands		West Germany		United States	
Models	Values t_1	Values t_2	Values t_1	Values t_2	Values t_1	Values t_2
Model A[a]						
Indicators						
4 item index t1	.59	.00	.68	.00	.60	.00
8 item index t1	.61	.00	.50	.00	.62	.00
4 item index t2	.00	.61	.00	.73	.00	.54
8 item index t2	.00	.67	.00	.58	.00	.55
ϕ_{21}	1.14		.74		.98	
χ^2_1	20.95		7.89		42.42	
p	.00		.005		.000	
Model B[b]						
Indicators						
4 item index t1	.66	.00	.63	.00	.49	.00
8 item index t1	.54	.00	.54	.00	.77	.00
4 item index t2	.00	.62	.00	.62	.00	.73
8 item index t2	.00	.62	.00	.69	.00	.41
ϕ_{21}	.62		.42		.26	
χ^2_1	21.36		8.87		14.69	
p	.000		.003		.000	
Model C[c]						
Indicators						
4 item index t1	.61	.00	.71	.00	.61	.00
8 item index t1	.59	.00	.48	.00	.61	.00
4 item index t2	.00	.61	.00	.72	.00	.55
8 item index	.00	.62	.00	.59	.00	.54
ϕ_{21}	.84		.47		.64	
χ^2_1	.65		5.16		1.74	
p	.420		.023		.187	

[a] For all countries, $\theta_{31} = 0$; $\theta_{42} = 0$.

[b] Netherlands: $\theta_{31} = .32$; $\theta_{42} = .36$. West Germany: $\theta_{31} = .30$; $\theta_{42} = .17$. United States: $\theta_{31} = .4$; $\theta_{42} = .4$.

[c] For all countries, $\theta_{31} = .2$; $\theta_{42} = .2$.

the absence of an unemployment item in both batteries, we would expect that the correlations between the value factors and inflation would be stronger than those for unemployment. The data support these hypotheses; confirmatory factor analyses reveal that inflation concerns are significantly ($p < .05$) and *negatively* related to the two value factors, with the size of the t_2 correlations being −.45 for the Netherlands, −.34 for West Germany, and −.54 for the United States. The comparable correlations for unemployment are significant and *positive*, with the values for the Netherlands, West Germany, and the United States being +.14, +.13, and +.20, respectively.

Summary

We have shown that the four-item battery regularly included in Euro-Barometer surveys to measure public value priorities does not show a significant trend in the direction of postmaterialist values in a number of Western European nations. Moreover, the measure is very sensitive to short-term changes in economic conditions that alter the public political issue agenda. The nature of this sensitivity is such that sharp increases in unemployment in the early 1980s make it appear, based on the four-item measure, that there was a substantial shift toward postmaterialism in several countries. Pooled cross-sectional time series analyses for eight of these countries over the 1976-86 period indicate that the failure to include an unemployment statement in the measure does much to account for the apparent postmaterialist trend.

These aggregate-level findings showing considerable short-term dynamism in responses to the four-item value battery are inconsistent with recent analyses of American, Dutch, and German panel data showing high levels of individual-level stability in underlying values. Thus, any argument that the value measures are problematic must address the question of individual-level change. Our reanalyses of these panel data demonstrate that results depend heavily on assumptions about error covariances between the value indicators. If one makes the reasonable assumption that they are present, considerable individual-level instability in underlying values as measured by the four- and eight-item value indices is apparent in two of the three cases. Also, the direction and magnitude of correlations between the underlying factors measured by the value batteries and concern with inflation and unemployment as current issues are consonant with our aggregate-level results. The individual-level findings thus support the argument that the four-item value battery is defective because of its strong sensitivity to short-term changes in the salience of inflation and unemployment on the issue agenda.

Recognizing the measurement difficulties associated with the four-item battery is important because data generated by it constitute the principal empirical base for many analyses of values and value change in contemporary Western countries. Since the evidentiary foundation provided by the battery is flawed, analyses based on it are problematic. Until better measures are employed in repeated surveys in these countries, conclusions about the nature and extent of value change in their populations should be treated with caution.

Notes

1. See, among others, Boeltken and Jagodzinski 1985; Dalton 1977, 1981; Flanagan 1979, 1982a, 1982b, 1987, 1990; Inglehart 1971, 1977, 1979, 1981, 1985, 1987, 1990a, 1990b; Jagodzinski 1984, 1986; Van Deth 1983a, 1983b.

2. The panel surveys are part of the *Political Action* project (see Jennings, Van Deth et al. 1990, Appendix A). We thank M. Kent Jennings for making these data available.

3. Data on the four-item value priority battery and the summary materialist-postmaterialist varia-

ble for the 1976–84 period are from the *European Communities Studies, 1973–84 Cumulative File* (ICPSR Study #8434). The 1985 data are averages from *Euro-Barometer 23* (ICPSR Study #8411) and *Euro-Barometer 24* (ICPSR Study #8513). The 1986 data are averages from *Euro-Barometer 25* (ICPSR Study #8616) and *Euro-Barometer 26* (ICPSR Study #8680).

4. As Flanagan (1982b, 116) argues, "a fair test of Inglehart's scale at the aggregate level, therefore, would be to chart its stability in a country during a period when the political salience of a number of the scale's options are undergoing substantial change." (See also Boeltken and Jagodzinski 1985; Flanagan 1987; Jagodzinski 1986.)

5. Unemployment was in single digits in all eight countries in 1980. By 1985 it had increased in every case, and in Belgium, Great Britain, Ireland, Italy, and the Netherlands it exceeded 10%. In West Germany unemployment was 8.3% in 1985—nearly two and one-half times greater than in 1980 (3.3%).

6. The results are not an artifact of collinearity among the economic variables. The correlation between the logged measures of unemployment and inflation is −.10, between unemployment and GDP, −.28, and between inflation and GDP, −.07.

7. The eight-item battery does not include an unemployment statement (see Inglehart 1990a, 132). This battery fares very poorly in turnover analyses: 68% of the Dutch, 63% of the Germans and 66% of the Americans change their top priority from one interview to the next.

8. Following Inglehart (1990a, 82) the four-item index ranges from 1 (pure materialist) to 3 (pure postmaterialist), and the eight-item index ranges from 1 (pure materialist) to 4 (pure postmaterialist).

9. An alternative approach to estimating the interfactor correlation is to assume that the factor loadings (λ_x's) for the same indicators are identical across the panels (Long 1983, 46). One can then free the error covariances, θ_{31} and θ_{42}, and estimate the interfactor correlation, ϕ_{21}, which is identified, although other parameters are not (Long 1983, 54–55). The ϕ's equal .88 for the Netherlands, .58 for West Germany, and .54 for the United States, thereby again showing that in the latter two countries a large percentage of the variance in the t_2 value factor is unexplained by its t_1 counterpart.

10. The question preface was, "Now we would like to know how you feel about some of the particular issues and problems that people often talk about these days." Respondents were given a list of 12 items and asked how the government is handling them, which five were most important, and the relative importance of the five selected. The measures of the importance of inflation and unemployment used here are three-point indices scored as follows: most important = 2; second-most important = 1; less important or not important = 0. The analyses reported are for models assuming a .20 error covariance for the values indicators. Changing this assumption does not materially affect the results. Changing the assumptions about the (stipulated) error variance in the inflation and unemployment indices (see Hayduk 1987, 118–22) also has negligible effects.

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CONTROVERSIES

CONCEPTUAL PROBLEMS IN THEORIZING ABOUT INTERNATIONAL CONFLICT

Axiomatic and deductive theorizing about international political conflict has precipitated lively debate. Much of the disputation of recent years has derived from an expected utility theory of conflict advocated by Bruce Bueno de Mesquita. In the June 1990 issue of this Review, *Roslyn Simowitz and Barry L. Price dissected crucial parts of Bueno de Mesquita's formulation of the theory, arguing that it suffers from errors of logic and clarity. In this controversy, James D. Morrow challenges the claims of Simowitz and Price that the reviewed theory is logically and conceptually flawed. In turn, Price and Simowitz join issue.*

In recent years, there has been considerable discussion regarding the applicability of rational choices models to the study of international conflict. Roslyn Simowitz and Barry L. Price (1990) claim to find logical flaws in a number of propositions contained in Bruce Bueno de Mesquita's (1981, 1985) work. Although they judge Bueno de Mesquita's work to be "the most exciting and ambitious effort" to develop an axiomatic deductive theory of international phenomena (p. 439), their criticisms, if correct, are quite damaging for the theory in *The War Trap.* They purport to show that seven of the nine propositions in *The War Trap* do not follow from his assumptions. If correct, they would demonstrate that Bueno de Mesquita's theory fails by his own criteria (Bueno de Mesquita 1981, 8–10).

However, their claims are incorrect. Simowitz and Price agree with Bueno de Mesquita that there are three criteria for judging scientific theories: (1) precision and clarity of concepts, (2) logical consistency, and (3) empirical content (pp. 443–44). The first two criteria correspond to their discussion of internal conceptual problems, and the third corresponds to their discussion of external conceptual problems and empirical problems. I shall focus on Simowitz and Price's discussion of internal conceptual problems in Bueno de Mesquita's work, because it is the heart of their critique.[1] They claim that Bueno de Mesquita's theory is logically inconsistent; but a closer examination shows that Bueno de Mesquita's failure is only in occasional lapses of clarity.

Simowitz and Price's analysis is mistaken in two fundamental ways. First, their critique of Bueno de Mesquita hinges on an assumption that *they* assert is central to his theory. Another assumption is more defensible as the major premise in the light of the development of Bueno de Mesquita's research program; his conclusions do follow from that other assumption. Second, they reword some of Bueno de Mesquita's propositions in ways that create logical problems within the theory. More plausible readings of the original eliminate these problems.

Ideally, theories should be stated with absolute clarity. Bueno de Mesquita's lack of clarity on several important issues contributes to the problems Simowitz and Price claim to find. But they note Laudan's observation that lack of clarity can be an advantage for theories in their early development: "Many theories often begin with vague and ambiguous concepts that work to their advantage in that they allow the theorist to extend the theoretical concepts to their broadest possible domain" (p. 445; Laudan 1977, 49). Their critique raises central questions about Bueno de Mesquita's research that require further articulation of his theory. I hope that this response clarifies some of these issues, helping those outside that research tradition to understand it better. Only through careful logical and empirical criticism can science advance, and I welcome Simowitz and Price's comments in that spirit. I begin with the question of Bueno de Mesquita's major premise and then turn to Simowitz and Price's misstatements of Bueno de Mesquita's propositions.

What Is the Major Premise?

The essence of rational choice is that actors compare the relative attractiveness of alternatives and choose the alternative they believe to be most attractive. Applying that idea to explain international conflict requires defining what alternatives the actors perceive and how they evaluate the attractiveness of alternatives. The major premise at the heart of the expected utility approach to international conflict is the following:

MAJOR PREMISE: *Actors facing decisions in crises choose the course of action they believe is best for them.*

In the theory presented in *The War Trap,* this premise is refined by three auxiliary assumptions that characterize the choices actors face and how they evaluate those choices. The major premise of a theory is the core assumption that directs the research program; auxiliary assumptions elaborate the major premise within a given theory but can be modified as the research program advances through a series of theories.

AUXILIARY ASSUMPTION 1. *Actors compare two options, war and the status quo (SQ), when deciding whether to go to war.*

AUXILIARY ASSUMPTION 2. *The status quo outcome (SQ) lies halfway between outcomes of victory and defeat for the initiator.*

AUXILIARY ASSUMPTION 3. *Utilities are normalized with $u(SQ) = 0$.*[2]

Simowitz and Price claim that Bueno de Mesquita's major premise is that positive expected utility is a necessary but not sufficient condition for war. The difference between my statement of Bueno de Mesquita's major premise and theirs is whether or not the theory must be restricted to necessary but not sufficient conditions for war in place of the more common search for necessary *and* sufficient conditions. Simowitz and Price maintain that Bueno de Mesquita's research program can identify only necessary but not sufficient conditions for war. The logical structure of deductions from my assumptions depends upon the auxiliary assumptions. Under some conditions, my statement of Bueno de Mesquita's major premise leads to necessary and sufficient conditions for war; under others, it produces only necessary but not sufficient conditions for war.

Bueno de Mesquita (1981) presents two arguments for searching for necessary but not sufficient conditions. First, the *gatekeeper argument* contends that every nation possesses one individual whose utility corresponds to the nation's expected utility (pp. 27–33). A nation's gatekeeper can block his or her state from going to war but cannot force it to go to war. Then positive expected utility is necessary but not sufficient for war because the gatekeeper will veto action whenever he or she prefers the status quo to war.[3] Second, the *other-options argument* asserts that options other than war and the status quo always exist (pp. 4–5). Nations might prefer one of these other options to war even when they prefer war to the status quo. This argument leads to necessary but not sufficient conditions for war because states with positive expected utility might choose some other as-yet-unmeasured option over war.

Under my statement of Bueno de Mesquita's major premise, the argument used to justify the derivation of necessary but not sufficient conditions is important. In *The War Trap*, Bueno de Mesquita does not choose between them. If the other-options argument is chosen, we can move toward necessary and sufficient conditions for war by including the other options in the theory. Bueno de Mesquita, Lalman and I expand the available alternatives beyond war and the status quo (Bueno de Mesquita 1985; Bueno de Mesquita and Lalman 1986; Morrow 1985, 1986). Indeed, Bueno de Mesquita discusses moving the theory toward necessary and sufficient conditions for war by including other options than war and the status quo (1981, 182–84). The theory presented in *The War Trap* is limited to necessary but not sufficient conditions for war because the other options are not specified. Then necessary but not sufficient conditions for war are not part of the major premise of the research program; they are a consequence of what other options are specified in a particular theory.

The question of the major premise directly addresses one of the logical fallacies Simowitz and Price claim to find. They argue that proposition 2 (Bueno de Mesquita 1985, 164), namely, that war is likely when both sides have positive expected utility for war, is inconsistent with the expected utility approach (p. 446). It is inconsistent with their major premise. However, Bueno de Mesquita precedes proposition 2 with a six-paragraph discussion on negotiated settlements (other options) as an alternative to war and the status quo. He argues that when both sides prefer war to the status quo, it will be very difficult to reach a negotiated settlement: "After all, how can adversaries compromise when both expect to be the recipient of net benefits?" (1985, 164). Proposition 2 follows from my version of the major premise because, by definition, the condition of the proposition (that both sides expect to win) eliminates the possibility of a negotiated settlement, the primary other option to war.[4] Similarly, propositions 3, 4 and 6 (Bueno de Mesquita 1985) and proposition 6 in *The War Trap* (which is the same as proposition 7 in Simowitz and Price [1990]) are consistent with this approach, contrary to Simowitz and Price's claim.

The choice of major premise here is critical. Simowitz and Price believe it is the statement that positive expected utility is necessary but not sufficient for war. But they admit that this presumption is only their belief: "Because we find this statement to be one of the most critical assumptions in Bueno de Mesquita's theory we represent it as his major premise" (pp. 445–46). But three of the four sources they cite as extensions of *The War Trap* (p. 439: Bueno de Mesquita and Lalman 1986; Morrow 1985, 1986) do not assume this as a major premise. Instead, we compare other options to war and the status quo using expected utility calculations, the essential point of my version of the major premise.[5] These analyses do not change the major premise of the research program; but they do remove the first auxiliary assumption.

It would be unfair to Simowitz and Price to insist that they read between the lines to determine the major premise in Bueno de Mesquita's work. Bueno de Mesquita does not state the major premise of his research program explicitly. But they argue not that Bueno de Mesquita is unclear but that his conclusions do not follow. As I have just shown, his conclusions *do* follow from my statement of the major premise, and it characterizes the central focus of analysis in this research program better than Simowitz and Price's version of it.

Misstatements of Propositions

Of the five logical problems Simowitz and Price claim to find in Bueno de Mesquita, only one arises directly from their version of his major premise. The other four arise because they simply misstate his propositions.

Simowitz and Price create logical problems by rewording Bueno de Mesquita's propositions in odd ways. First, they claim that Bueno de Mesquita tests an incorrect version of his proposition 1 (Bueno de Mesquita 1981). But their proposition 2 misstates his proposition 1. Bueno de Mesquita's exact wording is the following (because Simowitz and Price's numbering does not follow *The War Trap*, I code them BdM for Bueno de Mesquita, S&P for Simowitz and Price, and M for myself):

PROPOSITION 1-BdM. *Nonaligned states cannot rationally attack more powerful nonaligned states.* (Bueno de Mesquita 1981, 68)

Before turning to Simowitz and Price's restatement of this proposition, some concepts from *The War Trap* need to be identified. Bueno de Mesquita distinguishes two types of nations: initiators (nation *i*) and targets (nation *j*). The side that attacks first is the initiator of a war (Small and Singer 1982, 194–98). Simowitz and Price restate proposition 1-BdM as

PROPOSITION 2-S&P. *If i and j are nonaligned and $P^j > P^i$ then ($E^i < 0 \rightarrow$ no war), where P^j is the power of the victim and P^i is the power of the initiator (E^i is nation i's expected utility for war).* (Simowitz and Price 1990, 447)

A correct restatement of proposition 1-BdM using Simowitz and Price's notation would be as follows:

PROPOSITION 2-M. *If i and j are nonaligned and $P^i < P^j$, then i will not attack j.*

Bueno de Mesquita's test of this proposition compares the strength of initiators to targets in wars between nonaligned states (1981, 154–56). Simowitz and Price (p. 447) translate the tested hypothesis as "Conflicts among nonaligned states are initiated by the stronger adversary." In their logical notation, Bueno de Mesquita's test would be "If war and *i* and *j* are nonaligned then $P^i > P^j$."[6] This statement is just the contrapositive of proposition 2-M if we assume *i* and *j* are nonaligned.[7] Conditional sentences and their contrapositives are logically equivalent; testing the contrapositive is equivalent to testing the conditional. Contrary to Simowitz and Price's contention, there is no faulty logic here.

How do Simowitz and Price create this logical problem? They have changed the consequent of proposition 1-BdM to a reiteration of their major premise. This substitution appears innocuous, but its consequences are important and highly misleading. They render the antecedent of proposition 1-BdM irrelevant by stating the major premise as the consequent of the proposition. A conditional statement is false only when its antecedent (here, "The initiator and target are nonaligned, and the target is more powerful than the initiator") is true and its consequent ("no war" in this case) is false. Then conditional statements are always true when their consequents are true regardless of the truth of their antecedents.[8] Simowitz and Price's substitution trivializes Bueno de Mesquita's deduction into a restatement of their major premise. Returning to proposition 1-BdM, Bueno de Mesquita argues that a nonaligned nation *i* cannot attack a stronger nonaligned nation *j* because *i*'s expected utility calculation reduces to a bilateral calculation, which must be less than zero if nation *i* is weaker. To assert in the consequent that "if A's expected utility is negative, then war cannot occur" (as Simowitz and Price do) is redundant; the antecedent suffices to show that A's expected utility is negative, so war cannot occur. This derivation does depend upon a number of auxiliary assumptions beyond the major premise. Simowitz and Price do not criticize those assumptions.

Contrary to Simowitz and Price's claim (p. 447), Bueno de Mesquita does test proposition 1-BdM properly. Bueno de Mesquita's Table 5.25 (1981, 155) compares the initiator's capabilities to those of its opponents for all wars between nonaligned nations.[9] Of the 23 wars between nonaligned countries, 19 are initiated by the stronger side. There are four cases where nonaligned nations attacked other nonaligned nations with greater measured capabilities. Does this confirm or refute proposition 1? We do not know. The indicator of capabilities contains measurement error of unknown magnitude (Bueno de Mesquita 1981, 102). To determine whether these four cases refute proposition 1 or merely fall within the bounds of acceptable measurement error, we need some idea of the size of this measurement error. Bueno de Mesquita calculates the probability that 19 out of 23 initiators would be stronger if the chance of either side being stronger is equal (1981, 156). It is less than .002, but even this test does not perfectly address the measurement error issue. A better test would assess whether the observed capabilities of the initiators in the four cases are within reasonable measurement error of their opponents. Nevertheless, Simowitz and Price focus on their claim that Bueno de Mesquita's test does not follow from proposition 1-BdM rather than the potential problem of measurement error.

Second, Simowitz and Price claim that Bueno de Mesquita's test of his propositions 3, 3′, and 3″ on how allies can fight (Bueno de Mesquita 1981, 73–83 for the propositions, 159–64 for the test) does not follow from his assumptions. They render his test as "Alliances increase a nation's chances of being attacked by its friends" (p. 447). (Although Simowitz and Price put this line in quotation marks, they do not provide a citation for it; and I cannot find it in either of the relevant sections of *The War Trap.*) What Bueno de Mesquita actually tests is whether allied dyads fight more often than nonallied dyads (1981, 160–61). Simowitz and Price's misstated version of the proposition requires a test of the likelihood that friendly, nonallied dyads go to war. Bueno de Mesquita's test does not calculate this likelihood.

While this distinction may seem trivial at first glance, it is not. Simowitz and Price's statement of the tested proposition is as follows:

PROPOSITION 5-S&P. *If i and j are allies satisfying conditions x, y, and z, then ($E^i \geq 0 \rightarrow$ an increased probability of war between i and j).* (Simowitz and Price 1990, 447)

This statement is not the proposition Bueno de Mesquita tested; a correct statement of this test in the form of a proposition is the following:

PROPOSITION 3-BdM. *If i and j are allies satisfying conditions x, y, and z, then an increased probability of war between i and j exists.*

Again, Simowitz and Price change the consequent "an increased chance of war" to the conditional statement "$E^i \geq 0 \rightarrow$ an increased probability of war between i and j." Although Bueno de Mesquita's propositions 3, 3′, and 3″ do not directly state that allies are more likely to fight than nonallies, they are surrounded by an argument that makes exactly that point and is consistent with Bueno de Mesquita's theory. Bueno de Mesquita contends that the value of the status quo between allies can only deteriorate in the future, while between other nations it could improve, deteriorate, or remain the same (1981, 74–78).[10] War becomes more attractive relative to the status quo when it is expected to worsen in the future. Under that circumstance, allied dyads are more likely to have at least one member with positive expected utility for war than are nonallied dyads. If we assume that all nations with positive expected utility are equally likely to go to war, then allies should be more likely to fight than nonallies. This proposition requires this additional assumption that Bueno de Mesquita does not explicitly make. Nor does he state it explicitly as a derivation of his theory.

Third, Simowitz and Price reiterate their version of Bueno de Mesquita's major premise in their proposition 6 (restatement of Bueno de Mesquita's proposition 2). Bueno de Mesquita's proposition 2 states that it is possible for a nonaligned state to attack a stronger state with allies. To test this proposition, he has only to show the existence of cases where a nonaligned state attacked a stronger state with allies. He does this with a discussion of the Crimean War (1981, 71–72 and Table 5.25). However, most of Bueno de Mesquita's empirical analysis of this proposition examines initiators' expected utility across the alignment or nonalignment of both sides (pp. 156–59). This evidence is irrelevant for testing his proposition 2.

Simowitz and Price's version of Bueno de Mesquita's proposition 9 is the following:

> PROPOSITION 8-S&P. *The larger* $|E^i - E^j|$, *the more difficult it is to find a settlement and the more costly the war.* (Simowitz and Price 1990, 448)

Bueno de Mesquita's exact wording is as follows:

> PROPOSITION 9-BdM. *The greater the difference between what the initiator believes it can gain and what the opponent believes it must lose (including the possibility that the victim believes it can win), the more difficult it is to find a settlement that both sides can accept, and hence the costlier the war.* (Bueno de Mesquita 1981, 92)

Each side's expected utility embodies what it expects to gain from war if positive or lose from war if negative. The initiator believes it can gain E^i; the opponent believes it will lose $-E^j$. Because larger negative expected utilities imply greater expected losses, what the opponent believes it will lose increases as E^j decreases. An alternate rendition of proposition 9-BdM using Simowitz and Price's notation is the following:

> PROPOSITION 8-M. *The larger* $E^i - (-E^j)$ $(= E^i + E^j)$, *the more difficult it is to find a settlement and the more costly the war.*

This restatement captures the context of Bueno de Mesquita's argument (1981, 89–92) better than proposition 8-S&P and is logically consistent with his proposition 6 (1981). To their credit, they realize proposition 8-M is consistent with Bueno de Mesquita's proposition 6 and admit it in their note 7. The logical fallacy that they purport to find arises only out of their incorrect inclusion of absolute values in proposition 8-S&P.

Finally, Simowitz and Price also misstate Bueno de Mesquita's argument that long-distance wars are disproportionately initiated by major powers (his proposition 4). They paraphrase his argument as "in long-distance wars, the probability increases that $E^i < 0$ (initiator's expected utility for war) for weaker nations, while the probability that $E^i \geq 0$ for more powerful nations increases" (p. 448). This is incorrect. Bueno de Mesquita argues that the probability that $E^i \geq 0$ for more powerful nations decreases, not increases

(1981, 83–84). Because the power of stronger nations decays more slowly across distance than that of weaker nations, the expected utilities of stronger nations decline more slowly than those of weaker nations as the distance each must fight across increases. Hence, major powers will disproportionately initiate long-distance wars because lesser powers are very unlikely to have positive expected utility for a long-distance war, while major powers can. Bueno de Mesquita (1981, 164–66) tests this proposition by comparing the percentage of long-distance wars initiated by major powers to the percentage of neighborhood wars initiated by major powers and finds strong support for it.[11]

Conclusion

Clarity, logical consistency, and empirical content are the agreed criteria for judging theories (as opposed to research programs). Simowitz and Price have attempted the task of assessing the theory of Bueno de Mesquita (1981, 1985) using these criteria and should be commended for doing so. But the logical fallacies they claim to find are produced by their misstatements of Bueno de Mesquita's propositions and by his lack of clarity at that time about the major premise of his research. Simowitz and Price argue that many of Bueno de Mesquita's tests do not solve empirical problems because his propositions do not follow from his theory. I have shown that the propositions do follow from Bueno de Mesquita's theory. Furthermore, they confuse some irrelevant tests in *The War Trap* with those propositions, leading them to ignore appropriate tests presented there.

The War Trap is not a perfect book by any means, but it did initiate a fruitful research program. I agree with Simowitz and Price that the expected utility approach still presents conceptual and empirical problems for additional research. Their discussion of Laudan recognizes the need to continue to develop research programs even in the face of internal and external conceptual problems (p. 445). Indeed, the existence of these problems is the reason for continued research in this program. Scholars (including Bueno de Mesquita) continue to examine the assumptions and deductions of the theories, extend existing theory to derive and test new hypotheses, and develop improved measures of existing concepts. Research continues to improve the logical structure and empirical content of arguments espousing a rational choice approach to understanding international conflict.[12] I realize it can be difficult for those outside an active research group to understand all the arguments within that group. I hope this comment has helped others to understand the strengths and weaknesses of the arguments and evidence advanced in the expected utility approach.

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In "The Expected Utility Theory of Conflict: Measuring Theoretical Progress," we attempted to assess the progressiveness of Bruce Bueno de Mesquita's theory. Following a review of the literature on the concept of theoretical progress, we determined that the Lackatosian measure of scientific progress offered by Bueno de Mesquita was seriously flawed. We therefore chose instead to measure the progressiveness of Bueno de Mesquita's theory according to criteria provided by Lawrence Laudan. Laudan's criteria required us to examine the empirical and conceptual problem-solving abilities of Bueno de Mesquita's theory. An examination of the theory's conceptual problems led us in turn to uncover several serious internal conceptual problems plaguing Bueno de Mesquita's work in *The War Trap* (1981) and "The War Trap Revisited" (1985). Specifically, we found that propositions 4 and 8 were not valid logical deductions of the theory presented in *The War Trap.* We also found that while propositions 1, 2, 3, 3′, and 3″ were valid logical deductions, the research hypotheses used to test these propositions were not. We found, as well, that propositions 6 and 9 in *The War Trap* were not logically consistent with each other. Finally, we determined that propositions 2, 3, 4, and 6 in "The War Trap Revisited" were invalid logical derivations of the theory presented in this latter work.

Morrow argues that our analysis is mistaken in two fundamental ways. First, he argues that we have mistakenly identified the major premise of Bueno de Mesquita's expected utility theory of international conflict and that this failure to identify "the correct major premise" has led us to conclude wrongly that several of Bueno de Mesquita's propositions do not follow logically from his major premise. Second, he argues that we have represented several major propositions in Bueno de Mesquita's theory *in such a way as to create logical problems that would otherwise not exist in the theory itself.* We believe that Morrow is mistaken on both points. First, his argument that the expected utility theory of conflict is based ultimately on an assumption central to all rational choice theories in no way negates the fact that several of Bueno de Mesquita's propositions (1981, 1985) were not valid logical derivations of the theories presented. Second, Morrow's argument that we have created the logical problems we identified in several of Bueno de Mesquita's propositions by including confusing terminology implies that removal of this "objectional" terminology will also eliminate the logical problems we identified. As we demonstrate, however, removal of these terms leaves the propositions in question with precisely the same logical problems we identified originally. The clearest way to show that both of the arguments Morrow makes are incorrect is to apply them first to our analysis of *The War Trap* and then to our analysis of "The War Trap Revisited."

Morrow's major criticism of our analysis of *The War Trap* is that we have misstated Bueno de Mesquita's propositions in such a way as to create logical problems that would not exist otherwise. Specifically, he argues that our inclusion of $E^i > 0$ and $E^i < 0$ in the consequent of Bueno de Mesquita's propositions created the logical problems we claimed to find there. We believe Morrow is wrong here. The best evidence of this fact is that when we eliminate these terms from Bueno de Mesquita's propositions, the propositions are still plagued by the same problems we identified originally. The clearest example of this fact is seen in propositions 3, 3′, and 3″ in *The War Trap.* These propositions read as follows:

PROPOSITION 3. *If i and j follow essentially identical policies on all issues of relevance to each other or to others, so that* $U_{ij} = U_{ii}$ *and the multilateral lottery* $E(U_i)$ *for* $k_\ell = 0$, *then i's ability to wage a war against its close friend j depends only on* $E(U_i)_b$. *Since the sign of* $E(U_i)_b$ *is determined by the magnitude of* p_i *relative to* $1 - p_i$, $E(U)_i$ *must be greater than or equal to zero if* $p_i > 1 - p_i$, $E(U)_i$ *will be zero if no change in* U_{ij} *is anticipated and will be positive if any change is anticipated in* U_{ij}.

PROPOSITION 3′. *If i believes those jointly allied with i and j will support j against i, with all other states assumed to be indifferent, then i's ability to wage war against its erstwhile ally j depends only on* $p_i \geq (1 - p_i)$, *and* $|E(U_i)_b| \geq$ *|the multilateral lottery* $E(U_i)$ *for* k_ℓ|, *given that all k allied with i and j support j, and given i's beliefs about the future.*

PROPOSITION 3″. *If i believes those jointly*

> *allied with i and j will support i against j, with all other states assumed to be indifferent (given $U_{ii} = U_{ij}$), then i's ability to wage war against a stronger j depends on k's estimate of multilateral $E(U_i)$ across all relevant k_ℓ being greater than or equal to zero and on that estimate of $|E(U_i)$ for $k_\ell| \geq |E(U_i)_b|$, given that all k allied with both i and j support i, and i's beliefs about the future.* (Bueno de Mesquita 1981, 79)

The logical problems we found with these propositions were that (1) they were tautologous, (2) the hypothesis used by Bueno de Mesquita to test them was not logically equivalent to the propositions themselves, and (3) the hypothesis was not a valid logical derivation of the theory Bueno de Mesquita presented in *The War Trap*. The question, of course, is to what extent Morrow's arguments have shown these logical problems to be of our own making rather than Bueno de Mesquita's making.

Morrow does not directly address our claim that propositions 3, 3′, and 3″ are mere tautologies. But his general argument regarding our use of the E^i term in Bueno de Mesquita's work suggests that this tautology is of our own making. It therefore seems appropriate to consider the merits of Morrow's general argument regarding the first logical problem we identified in these propositions. Proposition 3 specifies three conditions that cause $E(U_i)$ to be either zero or positive; and propositions 3′ and 3″ specify conditions that cause $E(U_i)$ to be positive. For the sake of simplicity, we labeled these three conditions *x*, *y*, and *z*. According to each of these propositions, *i*'s ability to wage war against its ally *j* depends upon $E(U_i)$ being zero or positive. Because we interpreted Bueno de Mesquita's words "*i*'s ability to wage war against its ally" to mean *i*'s decision to wage war against *j*, we restated Bueno de Mesquita's propositions 3, 3′, and 3″: "If *i* and *j* are allies satisfying conditions *x*, *y*, and *z* → ($E^i \geq 0$ → war or no war).

We argued that these propositions were valid inferences from Bueno de Mesquita's central proposition but that they were valid only because each was tautologous. We argued that they were tautologous because their consequent—war or no war—is always true. If Morrow is correct that our inclusion of the E^i term in the logical statement of Bueno de Mesquita's propositions leads to the logical problem, then removing this term from the propositions should eliminate their tautologous nature. Removing the E^i term, we can restate Bueno de Mesquita's propositions as, *If i and j are allies satisfying conditions x, y, and z, then i may decide to attack j, or i may decide not to attack j.*

As was the case in our original statement of these propositions, the consequent of this statement—*i* may decide to attack *j* or *i* may decide not to attack *j*—is true regardless of its antecedent conditions *x*, *y*, and *z*. In other words, these propositions are tautologous. And judging from the fact that the tautology is equally apparent with or without the E^i term we used, Morrow's inference that we have created this logical problem through our misstatement of Bueno de Mesquita's propositions seems groundless.

The second logical problem we noted concerning propositions 3, 3′, and 3″ in *The War Trap* was that the hypothesis used by Bueno de Mesquita to test these propositions was not logically equivalent to the propositions themselves. The hypothesis used to test these propositions was, "Alliances increase a nation's chances of being attacked by its friends." Here, again, Morrow does not confront the issue directly. But considering the indiscriminate character of Morrow's criticism of our analysis, it seems appropriate to consider his general argument in reference to this logical problem as well. Remembering that we have already demonstrated that propositions 3, 3′, and 3″ are tautologous, we can dismiss Morrow's argument here simply by pointing out that whether one states Bueno de Mesquita's hypothesis with the E^i term, as we do, or without it, as Morrow does, the hypothesis tested is not a tautology. It therefore cannot be the logical equivalent of propositions 3, 3′, and 3″.

The third logical problem we found with propositions 3, 3′, and 3″ in *The War Trap* was that the hypothesis actually employed to test these propositions was not a logical derivation of the theory presented. Morrow responds directly to this criticism of Bueno de Mesquita's work. He begins his argument by noting what we identified as Bueno de Mesquita's own statement of his research hypothesis. By stating that he is unable to find the quote we have ascribed to Bueno de Mesquita, he suggests that we have put these words in the author's mouth. Hinting that Bueno de Mesquita did not render his tested hypothesis as "Alliances increase a nation's chances of being attacked by its friends," Morrow argues that "what Bueno de Mesquita actually tests is that allied dyads fight more often than nonallied dyads." He argues once again that a "correct statement of this test in the form of a proposition" should not include the confounding E^i term that we used. Rather, according to Morrow, a correct statement of Bueno de Mesquita's hypothesis should read as follows: *If i and j are allies satisfying conditions x, y, and z, then an increased probability of war between i and j exists.*

Having provided what he believes is a "correct" statement of Bueno de Mesquita's tested hypothesis, Morrow could now be expected to demonstrate that this hypothesis is in fact a valid logical derivation from the assumptions that Bueno de Mesquita provided in *The War Trap*. But this is not the line his argument takes. Instead, he tells us that this now correctly stated research hypothesis is a valid logical derivation of the theory Bueno de Mesquita presented in *The War Trap only if we rely upon an assumpton that Bueno de Mesquita did not state in this work.* Specifically, Morrow argues that "If we assume that all nations with positive expected utility are equally likely to go to war, then allies should be more likely to fight than nonallies."

We see two major problems with Morrow's argument here. The first is his suggestion that *we* may have created Bueno de Mesquita's description of his research hypothesis. In discussing the empirical tests of his hypotheses related to propositions 3, 3′, and 3″ Bueno de Mesquita says that

> Virtually no one, however, suggests that *alliances may*, even while possibly reducing the risk of external aggression, *increase a nation's chances of being* threatened or *attacked by its friends.* Yet the *expected-utility theory*—and almost any police blotter—*suggests that close, intimate interaction among friends often leads to violence. . . .* When allies fight, their friends and neighbors also are wary of intervening since they find choosing sides difficult. This would seem to be an important factor leading to the fact that *lovers quarrels are far more common than most other types of conflict.* I have suggested that *the same is true for nations* and now *I am prepared to put that proposition to the test.* (1981, 159–60; emphasis ours)

As this quote makes clear, our description of Bueno de Mesquita's view of the hypothesis he is testing is taken straight from the author's own words.

Much more problematic than Morrow's suggestion that we manufactured Bueno de Mesquita's description of his research hypothesis, however, is his argument that *if we assume that all nations with positive expected utility are equally likely to go to war,* the hypothesis Bueno de Mesquita tested is a valid logical derivation of the theory he presented in *The War Trap.* We addressed the issue of whether propositions presented in the two works (Bueno de Mesquita 1981, 1985) were valid logical derivations given the assumptions provided in these works. We concluded that several were not. Morrow does not address this criticism directly. Instead, he argues that the propositions in question *could be logically derived* if we used an additional assumption, which even he acknowledges Bueno de Mesquita never made. We believe this is another issue entirely. One issue concerns the theoretical validity of a theory as it was originally presented. Another concerns the possibility of salvaging an originally invalid deduction by employing additional assumptions. An unmistakable indication that Morrow is engaged primarily with the latter is his reliance on an assumption that he makes only now for a central part of his logical defense of Bueno de Mesquita's research hypothesis.

Just as Morrow passes over without comment the tautological character of propositions 3, 3′, and 3″, so also does he fail to comment on the central logical problem we identified with proposition 2 in *The War Trap.* We argued that the major problem with this proposition was that like propositions 3, 3′, and 3″, it also was a tautology. Proposition 2 in *The War Trap* reads as follows: *Under some circumstances, a nonaligned state can rationally attack a more powerful adversary, even though the adversary derives support from states that share its preferences.* In restating this proposition in logical terms, we represented *some circumstances* as *conditions x and y* and *can rationally attack* as *will or will not attack.* As was the case in restating a number of Bueno de Mesquita's propositions, we also inserted the E^i term that Morrow suggests is causing all the logical problems in Bueno de Mesquita's theory. Our exact proposition, then, reads as follows: *If i is nonaligned, j is more powerful than i and has allies, and conditions x, y are true, then* ($E^i \geq 0 \rightarrow$ *war or no war*).

Deleting the E^i term that Morrow objects to, Bueno de Mesquita's proposition 2 can now be restated as, *If i is nonaligned and j is more powerful than i, and has allies, and conditions x and y are true, then i may attack j or i may not attack j.* As is plainly seen, with or without the E^i term, this proposition is a tautology. Instead of explicitly acknowledging or contesting this claim, however, Morrow offers a short justification for the kind of evidence Bueno de Mesquita used to test this proposition. Specifically, he tells us that "to test this proposition, he has only to show the existence of cases where a nonaligned state attacked a stronger state with allies. He does this with a discussion of the Crimean War." Our central point, of course, was that being a tautology, Bueno de Mesquita's proposition was nonfalsifiable. The proposition is true even if there are no examples of cases in which *i* attacks *j.* Morrow's discussion about the appropriateness of Bueno de Mesquita's test of this hypothesis is irrelevant to our argument regarding the logical problem with this proposition.

Just as Morrow fails to directly address the central criticism we voiced regarding proposition 2 in *The War Trap,* so does he fail to squarely confront the crux of our argument concerning proposition 4. Bueno de Mesquita's proposition 4 holds that major powers disproportionately initiate long-distance wars. According to Bueno de Mesquita and Morrow this is because "the power of stronger nations decays more slowly across distance than that of weaker nations." In turn, this means that "the expected utilities of stronger nations decline more slowly than those of weaker nations as the distance each must fight across increases." Our major criticism of this proposition was that this inference was valid only if the utilities of the major power initiators for long-distance wars were initially greater than or equal to the utilities of nonmajor power initiators. We argued that this additional assumption was necessary because if the utilities of nonmajor powers are greater initially than those of major powers, then the expected utility values of nonmajor powers for fighting long-distance wars may be greater than the expected utility values of major powers even though the former decline more rapidly than the latter. Morrow again suggests that it is our incorrect paraphrasing of Bueno de Mesquita's argument that creates the problem we identified here. Yet he never addresses the fact that even after our paraphrasing has been "corrected," Bueno de Mesquita's proposition remains an invalid logical deduction.

While Morrow contends that it is our faulty paraphrasing and improper use of the E^i term in logical statements of Bueno de Mesquita's other propositions that have caused logical problems, he argues that the logical fallacy we find in propositions 6 and 9 from *The War Trap* is due to another culprit of our own making. Specifically, he argues that it is due only to our "incorrect inclusion of absolute values" in the logical restatement of proposition 9. As we noted in refuting Morrow's claims regarding the confounding character of the E^i term, assessing the merit of this argument is best accomplished by simply restating the appropriate propositions without the offending term. If Morrow is correct, elimination of the absolute value notation should restore propositions 6 and 9 to logical consistency. However, this does not appear to be the case because even when we delete the absolute values, we are still able to illustrate the inconsistency originally noted. Bueno de Mesquita's proposition 6 reads as follows: *If i's positive expected utility from a war with j is less than j's expected loss from a war with i* [*so that* $|E(U_i)| - |E(U_j)| < 0$], *both i and j are better off negotiating than fighting. Consequently, conflict will not rationally be permitted to escalate into war under these circumstances* (1981, 87). Deleting the absolute value sign as Morrow desires, Bueno de Mesquita's proposition 9 reads as follows: *The larger* $E^i - E^j$, *the more difficult it is to find a settlement and the more costly the war.*

Now suppose that we have two nations *i* and *j* at war in situation A:

$$E^i = .2,\ E^j = -.1$$

then

$$.2 - (-.1) = .3.$$

Suppose that during the course of this conflict, their expected utilities change to situation B:

$$E^i = .2,\ E^j = -.8.$$

then

$$.2 - (-.8) = 1.$$

According to Bueno de Mesquita's proposition 6, the change in expected utilities represented in situation B will quickly terminate the conflict through a negotiated settlement. This is because E^i has remained constant while the absolute value of E^j has become greater than E^i. According to proposition 9, on the other hand, the change in expected utilities represented in situation B will make a negotiated settlement between *i* and *j* all the more difficult and their continued conflict all the more costly. As this brief example illustrates, then, even after we remove the absolute values from proposition 9, there are still conditions under which this proposition is inconsistent with proposition 6.

Not only is proposition 9 inconsistent with proposition 6 for certain values of E^i and E^j, but it also appears to be logically inconsistent with what Morrow himself identifies as the major premise at the heart of the expected utility approach. Suppose, again, that we have two nations *i* and *j* at war in situation C:

$$E^i = 1,\ E^j = 1$$

then

$$E^i - E^j = 0.$$

In this situation j has positive expected utility for war, rather than the negative expected utility for war it had in situation B just discussed. According to the major premise of the expected utility approach identified by Morrow, therefore, j should prefer continued conflict in situation C rather than in situation B because j has a positive expected utility for war in situation C and a negative expected utility for war in situation B. Yet according to proposition 9, j prefers continued conflict with i in situation B rather than in situation C. This observation suggests that the logical problem we originally noted with propositions 6 and 9 could not possibly have been caused by our "misstatement" of proposition 9, as Morrow argues it was.

As noted at the outset, Morrow contends that we have represented several major propositions in Bueno de Mesquita's theory in such a way as to create logical problems that would otherwise not exist in the theory itself. By demonstrating that the logical problems we identified continue to plague Bueno de Mesquita's work even after the offending terms have been removed, we have shown that Morrow is wrong on this issue. There is nonetheless one case in which our restatement of a Bueno de Mesquita proposition did in fact create the logical problem we identified, although not for the reason Morrow suggests. We originally argued that proposition 1 of *The War Trap* was a valid derivation of Bueno de Mesquita's theory. We argued, however, that when this proposition was restated as a hypothesis, it was not logically equivalent to proposition 1 and was not a valid derivation. In response to Morrow's argument, we have reread Bueno de Mesquita's analysis of the hypothesis several times, and conclude that Morrow is correct on this point. Contrary to Morrow's general argument, however, it is not our inclusion of the E^i term that created the initial confusion. We believe Bueno de Mesquita's own lack of clarity to be a major contributing factor.

So far, we have considered Morrow's arguments only as they apply to our analysis of *The War Trap.* Morrow also argues that our failure to correctly identify the major premise of Bueno de Mesquita's work has led us to conclude incorrectly that several propositions in "The War Trap Revisited" (1985) are not valid logical derivations of the theory he presents there. We identified the central premise of both works as *Positive expected utility is a necessary but not sufficient condition for war initiation* because Bueno de Mesquita himself so indicates. Prior to testing his propositions in *The War Trap* Bueno de Mesquita writes:

> The broadest and seemingly obvious generalization that emerges from the theory is the expectation that wars (or other conflicts) will be initiated only when the initiator believes the war will yield positive expected utility. *This simple generalization,* based on the notion that people do what they believe is in their best interest, *provided the means to deduce several counter intuitive propositions about war.* (1981, 127; emphasis ours)

Bueno de Mesquita's assertion that wars will be initiated only when the initiator believes the war will yield positive expected utility does not clearly say whether positive expected utility is a necessary condition or a necessary and sufficient condition for war. But we argued that Bueno de Mesquita's central premise in *The War Trap* stipulated only necessary conditions for war because Bueno de Mesquita himself writes in the preface, "Chapters one and two elaborate the theoretical assumptions I make in constructing a theory of necessary, but not sufficient conditions for conflict" (1981, x).

In explaining why he limits his study to necessary but not sufficient conditions for conflict in *The War Trap,* Bueno de Mesquita states:

> After the necessary conditions that must be satisfied are met, other largely idiosyncratic—or even chance—factors must also fall into place. Put somewhat differently, the set of conditions suffi-

> cient for war may be so large that its specification is virtually impossible, but the set of necessary conditions may be small enough to be specified and encompassing enough to prove useful in discriminating between candidates for war and candidates for peace. (1981, 4-5)

Giving no indication that the set of conditions sufficient for war was any smaller or less idiosyncratic than it was when he penned *The War Trap,* Bueno de Mesquita once again tells us, immediately prior to deriving additional propositions in "The War Trap Revisited,"

> Since I am still modeling only necessary, and not necessary and sufficient conditions I can reiterate that decision maker *i can, but need not,* choose conflict as a strategy for acquiring policy gains (or reducing anticipated losses) only if equation (6) (i.e., $E^i(U_{ij})$) is not less than zero. (1985, 163, emphasis ours)

We took this clear statement by Bueno de Mesquita as an indication that even in his latter work he was still operating under a major premise that would allow him to stipulate only necessary rather than necessary and sufficient conditions for war.

Morrow acknowledges, of course, that Bueno de Mesquita restricts his theory to stipulating necessary but not sufficient conditions for war initiation in *The War Trap.* However, he argues that we have erred in attributing a central premise to Bueno de Mesquita that restricts his theory to necessary but not sufficient conditions for war initiation in "The War Trap Revisited." What, according to Morrow, is the "real" central premise of the theory Bueno de Mesquita (1981, 1985) presented? He doesn't say. Instead, he tells us that a much less restrictive premise is "at the heart of the expected utility approach." According to Morrow this premise is, *Actors facing decisions in crises choose the course of action they believe is best for them.*

Offering this "corrected" major premise, Morrow then argues that if we (1) define actors as nations, and define *choose the course of action they believe is best for them* as "choose war when their expected utility for war is greater than their expected utility for any other option"; (2) drop what Morrow calls "auxiliary" assumption 1, namely, that actors compare only two options—war and the status quo—when deciding to go to war; (3) accept the second and third auxiliary assumptions that Morrow provides; and (4) accept a "point that follows from Morrow 1985" (see n. 4)—then propositions 2, 3, 4, and 6 in "The War Trap Revisited" would not be "inconsistent with the expected utility approach."

We see several problems with Morrow's argument. First, it changes the question at issue from What constitutes the major premise of the theory Bueno de Mesquita presented in the two works to What constitutes the major premise of the expected utility approach? And as we clearly indicated in our original review of Bueno de Mesquita's work, our analysis never addressed the central premise of the expected utility approach or the logical validity of the many different propositions that are now encompassed by this approach. We examined only the deductive logic of the theory Bueno de Mesquita developed in *The War Trap* and "The War Trap Revisited."

Just as we did not argue that Bueno de Mesquita's propositions were inconsistent with the expected utility approach, we also did not argue that the propositions in question were "inconsistent" with what we identified as the major premise of Bueno de Mesquita's work. We argued only that these propositions were not valid logical derivations from his central premise. Logical consistency and logical validity, of course, are two very different concepts. Even though proposition A does not logically imply proposition B, proposition B can still be consistent with proposition A. Bueno de Mesquita (1985) did not claim, however, that he was testing propositions that were merely consistent with his major premise. He claimed instead to test propositions that were logical derivations of his theory. It was therefore the issue of logical validity we addressed, not the issue of logical consistency Morrow now raises.

It is possible that Morrow's substitution of the concept of logical consistency for logical validity is no more than a lapse of clarity on his part. If so, we still believe Morrow is wrong to argue that propositions 2, 3, 4, and 6 are logical derivations of the theory Bueno de Mesquita (1985) provided. What Morrow has identified as "the major premise at the heart of the expected utility approach" is in fact more accurately described as the major assumption of the rational choice approach. And while it is basic to all theories within the rational choice approach, including the theory Bueno de Mesquita (1981, 1985) presented, it is not the central premise of any particular theory within that approach. To state that actors facing decisions choose the course of action they believe is best for them by itself tells us nothing about the conditions under which nations will initiate international conflict. Indeed, the premise as it stands does not even incorporate the concepts of international conflict or expected utility. How, then, can it be the central premise of a theory of expected utility of conflict?

Moreover, even if the assumption we see as the major premise of the rational choice approach is in fact the central premise of the expected utility theory of conflict, Morrow is wrong to suggest that it would allow Bueno de Mesquita logically to derive the propositions in question. Our analysis of Bueno de Mesquita's work addressed the issue of whether propositions 2, 3, 4, and 6 (1985) were valid logical derivations given the assumptions Bueno de Mesquita provided therein. We concluded they were not. Morrow's argument does not directly address this criticism. Instead, he argues that the propositions in question could be logically derived if we used additional and different assumptions than those Bueno de Mesquita used originally. In other words, Morrow's argument is more about whether Bueno de Mesquita's propositions are valid deductions from an alternative set of assumptions rather than about whether these propositions were valid logical deductions as originally presented. The best evidence of this is the fact that both "auxiliary" assumption 2 and a critical point in the proof Morrow offers come from an article that was neither footnoted nor discussed in "The War Trap Revisited."

By design, we never addressed the issue that Morrow raises here. We see nothing wrong with his attempt to reduce or eliminate the logical problems in the theory of international conflict presented by Bueno de Mesquita (1981, 1985). According to Laudan, however, in order for a theoretical modification or adjustment to be acceptable, it must solve the problem in question without either reducing the empirical problem-solving capabilities of the theory or creating additional conceptual problems (1977, 116–17). The specific logical problem we identified with propositions 2, 3, 4, and 6 of "The War Trap Revisited" was that these propositions were not valid derivations of the theory's assumptions. The question before us now is whether the alternative assumptions provided by Morrow logically imply these same propositions. To assess this claim we need to look closely at the arguments Morrow presents in defense of proposition 2. This proposition reads as follows: *If i and j each believes their dispute falls in quadrant 1 of figure 3 (if both nations i and j have expected utilities greater than or equal to zero), then the probability that their serious dispute will escalate to war will approach 1.0* (Bueno de Mesquita 1985, 164). Morrow argues that this proposition is a valid logical deduction from the following assumptions:

1. Actors facing decisions in crises choose the course of action they believe is best for them.
2. The status quo outcome lies halfway between outcomes of victory and defeat for the initiator (Morrow 1985).
3. Utilities are normalized with $U(SQ) = 0$.

We believe Morrow is wrong here. He is wrong because none of Morrow's assumptions rule out the possibility that nations *i* and *j*, each expecting positive net benefits from war, will not find even greater positive net benefits from some Pareto-optimal compromise short of war. Morrow argues that proposition 2 follows from the major premise because "by definition, the condition of the proposition (that both sides expect to win) eliminates the possibility of a negotiated settlement, the primary other option to war." In the accompanying note Morrow tells us, "This point follows from Morrow (1985, 485-86)." But even there, Morrow acknowledges that "there must exist a compromise that is preferable to fighting to both sides" (p. 486). The most that can be argued in the situation where both nations expect to win is the argument Bueno de Mesquita himself presents in the discussion he provides immediately prior to proposition 2 (1985), namely, that adversaries will find compromise "very difficult" under these conditions. But, if compromise is a possibility—albeit a difficult one for nations in this condition—how can we logically deduce that war is a near certainty?

Conclusion

Morrow acknowledges that Bueno de Mesquita failed to "explicitly state his major premise" and various other assumptions employed to derive propositions claimed as deductions in both *The War Trap* and "The War Trap Revisited." Morrow views this problem, however, as "occasional lapses of clarity" on Bueno de Mesquita's part. Further, Morrow treats these lapses in clarity as equivalent to *conceptual ambiguity*, which we had already dismissed as common but not crippling for theories in their early stages of development. We believe, however, that the *logical* ambiguity plaguing Bueno de Mesquita's theory is a radically different and far more serious problem than the conceptual ambiguity to which Morrow alludes. Specifically, we believe that a failure to state explicitly the assumptions of a theory—let alone the major premise from which deductions are claimed—amounts to not having presented a deductive theory at all. Ambiguous deductions are more akin to invalid deductions than they are to ambiguous concepts, as Morrow suggests, and pose far more serious conceptual problems for a theory.

Equating conceptual ambiguity and deductive ambiguity is only one of several inaccurate and confusing equivalencies appearing in Morrow's critique. Throughout his critique, Morrow also moves back and forth between logical validity and logical consistency, treating them as though they were synonymous. At one point, for example, he says that we criticized propositions 2, 3, 4, and 6 in "The War Trap Revisited" because they were "inconsistent with the expected utility approach." Later in Morrow's discussion of these same propositions, however, he claims that we criticized them because they were not valid derivations of the theory. Logical validity involves the question of whether or not a theory's conclusions are logically implied by the theory's assumptions. Consistency, on the other hand, involves the question of whether or not a theory's assumptions imply contradictory statements. Morrow moves back and forth between consistency and validity, treating them as though they are logically equivalent when they are clearly not.

Morrow also incorrectly regards the expected utility theory of conflict as equivalent to the expected utility approach. We never argued, for example, that propositions 2, 3, 4, and 6 were "inconsistent with the expected utility approach." We argued only that they were not logical derivations of the expected utility theory of conflict Bueno de Mesquita (1981, 1985) provided. We stated immediately prior to our analysis of the logical validity of Bueno de Mesquita's propositions, "We include the assumptions and conclusions only of *The War Trap* and "The War Trap Revisited" because the related works of Bueno de Mesquita and others are not part of the deductive framework of his (Bueno de Mesquita's) theory" (1990, 445). This statement, as well as another similarly restrictive statement (p. 458, n. 3) should make it clear that we limited our analysis only to the two works and explicitly refrained from a broader analysis of either Bueno de Mesquita's later work or the expected utility approach in general.

Morrow maintains that "more plausible readings of the original" eliminate the problems we claim to find. He says that he hopes his response clarifies some of these problems, "helping those outside that research tradition to understand it better" and that he realizes "it can be difficult for those outside an active research group to understand all the arguments within that group." Now one of the reasons we gave in our original article advocating further pursuit of Bueno de Mesquita's theory to study conflict was its reliance on deductive reasoning (p. 456). We argued that a reliance on deductive reasoning and mathematical proofs advances theoretical progress, in part, by facilitating the exposure of faulty reasoning and invalid inferences. When deductive logic is used by a research program to generate propositions, it should not be necessary for those of us not in the research program to require guidance by members of the program to follow their "deductive logic." Neither should the propositions or the rules of inference employed be subject to multiple readings and interpretations. Indeed, the beauty of deductive reasoning is that both members and nonmembers of a research program can discover refutations and invalid inferences without any guidance from the program's active members.

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Notes

James Morrow thanks Bruce Bueno de Mesquita, Keith Krehbiel, David Lalman, Cliff Morgan, Robert Powell, Randolph Siverson, Harrison Wagner, and Barry Weingast for their comments.

1. Simowitz and Price's comments about empirical problems with the expected utility approach mainly repeat points others, particularly Nicholson (1987), have already made.

2. Both Wagner (1984, 410–11) and Nicholson (1987, 348–51) point out problems with the treatment of the status quo outcome in *The War Trap.* I show that Bueno de Mesquita (1981, 1985) assumes that the status quo is halfway between victory and defeat (Morrow 1985, 481). Without normalizing the utilities, the concept of positive expected utility would be nonsense. Utility is only determined up to a positive linear transformation. Without normalizing the utilities, we could always add a constant to them, creating any desired expected utility. Wagner and Nicholson both contend that Bueno de Mesquita's utility terms appear to give the difference between the status quo and winning or losing. *Assuming normalized utilities with the status quo* always halfway between winning and losing eliminates these problems.

3. Wagner points out a number of problems with the gatekeeper argument (1984, 408–10).

4. This point follows from Morrow (1985, 485–86).

5. Bueno de Mesquita and Lalman derive from their framework that necessary and sufficient conditions for war exist only under special conditions (1986, 1117).

6. Simowitz and Price confuse the issue further

by restating this test as their proposition 3—*If i and j are nonaligned and* $P^i > P^j$, *then* ($E^i \geq 0 \rightarrow$ war) (p. 447). This proposition states that in all pairs of nonaligned nations, the stronger will attack in every crisis. But this is not what proposition 1-BdM states nor what Bueno de Mesquita tests (1981, 154–56).

7. The contrapositive of the conditional statement "a implies b" is "not-b implies not-a." I ignore cases where *i* and *j* are equally powerful.

8. An equally valid deduction of this form would be, "If the moon is made of green cheese, then little green men from Mars will not attack the Earth if they have negative expected utility for the resulting war." The composition of the moon is irrelevant to the truth of this statement if we assume that positive expected utility is a necessary condition for war (including interplanetary war).

9. Oddly, though, Bueno de Mesquita refers in the text to the power comparison across all conflicts, not just wars. Wars alone form the appropriate test of proposition 1.

10. Bueno de Mesquita makes this argument by folding the benefit of preventing a future change in the status quo into the expected utility for war. As Nicholson points out, Bueno de Mesquita (1981, 1985) still needs some value for the present status quo (1987, 350–51).

11. He also includes comparisons of the expected utilities and outcomes for long-distance wars, which is irrelevant for testing his proposition 4.

12. Of course, one need not follow in the tradition of Bueno de Mesquita's work to adopt a rational choice approach to international conflict; two examples are Niou, Ordeshook, and Rose 1989 and Powell 1990.

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INDUSTRIAL CONCENTRATION, COUNTRY SIZE, AND TRADE UNION MEMBERSHIP

In his article, "Union Organization in Advanced Industrial Democracies" in the June 1989 issue of the Review, *Michael Wallerstein advocated a model to account for cross-national differences in trade union organization rates. He argued that the size of the labor force provided the most important determinant of variation in union density. In this controversy, John Stephens takes issue with the operationalization of a key variable in Wallerstein's model—industrial infrastructure. Stephens reanalyzes the data using an alternative measure of this variable. His reanalysis supports his claim that in fact, the two variables yield results that are statistically indistinguishable. Wallerstein responds.*

Recent work on the political economy of advanced capitalist democracies has demonstrated that the difference in the level of union organization is causally linked, either directly or indirectly, to some of the most important differences in the political economies of these countries. It has been directly linked to the equality of income distribution and the strength of parties of the Left (Cameron 1978; Korpi 1983; Stephens 1979). The relationship with the strength of socialist parties links it indirectly to the level of welfare state expenditure, the distributive impact of public spending, and the degree of decommodification achieved by welfare legislation (Cameron 1978; Esping-Andersen 1990; Hicks and Swank 1984, 1990; Korpi 1983, 1989; Stephens 1979). Union power as indicated by a combination of union density and centralization has also been found to be strongly related to various measures of economic performance in the 1970s and 1980s (Cameron 1984; Hicks 1988; Lange and Garrett 1985). Finally, union strength has been linked to the development of economywide bargaining, or "corporatism" (Maier 1984; Stephens 1979).

It is surprising, then, that there are so few attempts to account for the differences in the level of union organization among advanced capitalist democracies. There is a large empirical literature on the dynamics of union growth, that is, on what causes union membership density to vary through time in a given country (see Kjellberg 1983, chap. 1; Miller 1988 chap. 2; Wallerstein 1989, 482–84 for reviews of this literature). However, this literature yields little in the way of explanation of the very large differences in union density across countries. Indeed, until Wallerstein's (1989) recent contribution, the only systematic attempts to account for the causes of variation in the density of union organization across a large number of cases were Kjellberg's (1983) comparative historical analysis and my cross-national quantitative analysis (1979, 40–46, 109–17).

All three of these studies agree that there is a feedback effect between union organization and labor political power—

that is, strong unions are effective mobilizing agents for parties of the Left, and socialist parties, once in power, facilitate union organization. The differences in socialist incumbency would appear to be the single most important reason for the increasing differences in union density across countries. In my analysis (1979), the other variables shown to be associated with union density were ethnic and linguistic diversity and, following Ingham (1974), the character of the industrial infrastructure—above all, the degree of economic concentration. The relationship between the level of organization and degree of economic concentration explains a fact that must strike anyone who casually eyes comparative data on union density: the degree of organization in small countries is much higher than in large ones. Though Kjellberg (1983) is critical of the weight I gave to this variable, he appears to agree that at least some of the differences between countries at an early period of union development were due to the character of the industrial infrastructure. At any rate, he offers no other explanation for the correlation between size and degree of organization.

Wallerstein (1989) offers a quite different explanation of why small countries are more organized than large ones. Based on a mathematical derivation, he argues that the single most important determinant of cross-national variation in union density is the size of the labor force. He then proceeds to test his explanation against competing explanations and concludes that his own explanation fared far better in the statistical analysis. However, I contend that Wallerstein's results are a product of the improper operationalization of the key variable in my earlier analysis. Wallerstein operationalizes the character of the industrial structure with the proportion of the labor force in mining, manufacturing, and construction. In contrast, following Ingham (1974), I operationalized it with the average level of economic concentration in the economy.

Since neither explanation is intuitively obvious, I first explore the logic of the two explanations. I then reanalyze Wallerstein's data using the alternate operationalization. The reanalysis reveals that the two variables—size of the labor force and economic concentration—yield results that are statistically indistinguishable.

Why Are Small Countries More Organized Than Large Ones?

The two hypotheses offer different explanations for essentially the same fact: the level of organization is strikingly higher in small countries than in large ones. To develop his explanation, Wallerstein uses bargaining theory to model the impact of union density on the outcome of collective bargaining. His argument assumes that unions and firms are perfect agents of their constituents, maximizing the expected income of union members and profits, respectively. The power of a union in bargaining depends on its ability to disrupt the supply of labor, that is, to carry out an effective strike. This depends in part on the ability of employers to hire replacement workers during a strike. Mathematically, Wallerstein shows that "as union density increases, a strike becomes more threatening . . . and union members obtain more of the joint surplus" (p. 486). Thus, unions will want to increase the level of unionization. However, the recruitment of new members costs something; and whether unions will pay this cost depends on whether the returns exceed the payout.

Wallerstein then proceeds mathematically to develop four propositions about various factors that affect unions' decisions to devote more resources to organizing. The first three make sense both intuitively and from the point of view of existing knowledge about the factors that affect union organizing. These need not concern us here because they do not explain the statistical result under consideration here. The fourth proposition states that the "target level of unionization declines as the size of the labor force increases" (p. 488). This is true because, according to the model, the gains in collective bargaining from organizing more workers depend on the proportion of the relevant workforce to be organized, while the costs depend on the absolute number of workers to be organized.

Drawing on Ingham's (1974) discussion of the effect of the industrial structure on bargaining centralization, I offer three hypotheses.

1. Among industrial countries, the size of the population is the main determinant of the size of the domestic market, which, in turn, is the main determinant of the degree of industrial concentration. Industrial concentration is an important determinant of union density.
2. Late and rapid industrialization causes countries, particularly small ones, to have less technically and organizationally complex industrial structures. This aids unionization.
3. Late industrialization in small countries leads to greater product specialization. Greater product specialization also facilitates unionization.

The logic behind these hypotheses follows. Small countries had to base industrialization on export industries because the domestic market was too small. Sharp competition in export industries forced out the smaller companies, which could not contend with the fluctuations of the world market. Furthermore, economies of scale imply that there is a limit to how small any firm can be and still be competitive. A firm of this smallest possible size will employ far more people proportionately in a small country than in a large one. For these same reasons, size is related to product specialization. Late industrialization (relative to other advanced capitalist countries) is associated with product specialization because highly specialized products assume the presence of an export market. Early industrializers like England had to produce a highly differentiated range of products aimed at the domestic market.

Thus, small countries, particularly if they industrialize late, are characterized by high product specialization and great concentration. Consequently the economy is characterized by a small number of noncompeting oligopolistic employers. Ingham (1974) contends that this type of industrial structure facilitates centralized employers' organization. There are five reasons why such an industrial structure might be thought to give rise to higher levels of unionization. (As in Wallerstein's argument, the first four are based on the two assumptions that unions will attempt to maximize current members' interests and would organize everyone if organizing were costless.)

First, there are economies of scale in organizing; it is less costly to organize all workers in a sector with four firms than all workers in a sector with a hundred firms, all other things equal. Unions and union organizers think of their goals as to organize plants, then firms, and industries; they rarely speak in terms of organizing individual workers.

Second, if unions are operating in a market that is regionalized (e.g., construction), they will have much less incentive to organize workers in the same industry in other regions. Since small countries characterized by high levels of capital concentration have fewer purely local or regional industries, this may contribute to higher levels of unionization in these countries.

Third, such an industrial structure will encourage the development of sectorwide unions (e.g., metalworkers' unions rather than separate autoworkers' unions, steel-workers' unions, etc.) and thus the organization of the remaining unorganized workers in a given sector.

Fourth, part of the effect of industrial structure on union strength may be mediated by the development of strong and centralized employers' organizations. To take Sweden as an example, Swedish employers developed a highly centralized and aggressive federation, the Swedish Employer's Federation (SAF), in the first decade of this century in order to fight the emergent labor movement. SAF economically supported firms involved in industrial conflicts and encouraged lockouts and other aggressive tactics vis-à-vis unions. As a response, the Swedish Confederation of Trade Unions (LO), the trade union confederation, was forced to centralize in order to avoid being crushed and, subsequently, to organize virtually all workers employed by SAF members in order to represent their own members effectively.[1] To generalize, the development of centralized employers' federations lowers the effectiveness of strikes (at current levels of organization) and thus raises the incentive of unions to organize firms in the employers' association.[2] It also forces a centralization of unions at the economywide level. This, in turn, makes the constituency of the unions *all workers in the country*, not just the workers in a given industry. In this situation, organizing a previously unorganized industry is no longer a public good benefiting the movement as a whole (e.g., increasing the movement's political clout), but not in the interest of an individual union to provide. Finally, Ingham points out that strong employers' associations tend "to produce 'visible' power relations and clear and specific points of conflict" (1974, 42), which, Miller contends, encourages unionization and union militancy (1988, 43).

My second hypothesis points to features of the industrial structure not related to the concentration. Late and rapid industrializers tend to skip the workshop stage of production and proceed directly to industrial mass production. Workshop production is associated with high differentiation of skills and, as a result, the development of craft unions. Craft unionism is based on the tactic of limiting the labor supply in a particular trade rather than organizing all workers in an industry. Thus, the strength of craft unionism is inversely related to the level of labor organization. This organizational complexity created by the technical complexity of workshop production often survives the workshop stage and continues to impede the organization of labor.

The Data Analysis

The data sets for the two analyses were similar but not identical. Wallerstein's sample was 20 of the 21 advanced industrial societies that have been democracies since World War II. Luxembourg was excluded due to lack of data on the dependent variable. My sample was 17 of these 20; Israel, Iceland, and Japan were excluded. Wallerstein's analysis was conducted on cross-sectional data for the late 1970s; my analysis was on data for 1970 (the analysis below includes 1930). The measures of left-wing government differed slightly.

In order to ensure that the differences in the analyses were not a product of the time period chosen, the countries included, or the operationalization of the variables, the analysis here will be carried out on a single data set. The sample is the 20 countries studied by Wallerstein.[3] His data will be used for all the variables present in his analysis. The dependent variable is union members as a percentage of the total number of wage- and salary-earners plus the unemployed measured between 1975 and 1980, with most of the data for 1979 (see Table 1). The size of the labor force is measured by the natural log

Table 1. Union Membership and Its Correlates

Country	Union Membership	Size of Labor Force	Index of Left-Wing Government	Ethnic Diversity	Linguistic Diversity	Economic Concentration
Sweden	82.4	3,931	111.84	.08	.12	1.55
Israel	80.0	997	73.17	.67	.20	1.71[a]
Iceland	74.3	81	17.25	.54	.05	2.06[a]
Finland	73.3	2,034	59.33	.16	.35	1.56[a]
Belgium	71.9	3,348	43.25	.55	.51	1.52
Denmark	69.8	2,225	90.24	.04	.12	1.52[a]
Ireland	68.1	886	.00	.04	.44	1.75[a]
Austria	65.6	2,469	48.67	.13	.01	1.53[a]
New Zealand	59.4	1,050	60.00	.37	.07	1.64[a]
Norway	58.9	1,657	83.08	.04	.19	1.58[a]
Australia	51.4	5,436	33.74	.32	.03	1.37[a]
Italy	50.6	15,819	.00	.04	.09	.86
United Kingdom	48.0	25,757	43.67	.32	.04	1.13
Germany	39.6	23,003	35.33	.03	—	.92
Netherlands	37.7	4,509	31.50	.16	.19	1.25
Switzerland	35.4	2,460	11.87	.50	.50	1.68
Canada	31.2	10,516	.00	.75	.48	1.35
Japan	31.0	39,930	1.92	.01	.02	1.11
France	28.2	18,846	8.67	.26	.14	.95
United States	24.5	92,899	.00	.50	.25	1.00

[a]Estimates on the basis of the logged gross domestic product.

of the number of wage- and salary-earners plus the unemployed.

Three variables from my previous analysis are included. Ethnic diversity is measured by the Atlas Naradov Mira index and linguistic diversity by the Muller index (Taylor and Hudson 1972, 271–74). The hypothesis is that it is more difficult to organize workers in ethnically and linguistically diverse societies.

Many features of the character of the industrial infrastructure do not lend themselves to easy operationalization. However, comparable data for 11 of the 20 countries for the most important variable, economic concentration, have been developed by Pryor (1973, 199–205). His measure for industrial concentration is based on the four-firm concentration ratio, that is, the percentage of employment, shipments, or production accounted for by the four largest enterprises in a narrowly defined industry. Using pairs of similarly defined industries for the United States and each foreign country, Pryor computes a weighted average of the overall concentration of the two countries, with the foreign countries' average concentration expressed as a ratio to the U.S. average.

To estimate the degree of concentration for nine cases of missing data, I followed the procedure developed in my previous analysis. The main determinant of the degree of concentration is the size of the domestic market (Ingham 1974; Pryor 1973), and Pryor's index is strongly correlated with the natural log of the gross domestic product (GDP), a measure of the size of the domestic market ($r = .74$). The degree of concentration for the nine missing cases was estimated by using the regression equation with the natural log of the GDP as a predictor.

The results of the analysis are shown in Table 2.[4] Because of multicollinearity,

Table 2. Determinants of Union Density, Late 1970s

Variables	Equation 1	Equation 2	Equation 3	Equation 4
Constant	98.15***	2.39	99.14***	6.64
Size of labor force (logged)	-6.48***	—	-6.31***	—
	-.56	—	-.55	—
Economic concentration	—	29.42***	—	30.42***
	—	.51	—	.52
Left-wing government	.28***	.28***	.26***	.24***
	.49	.50	.47	.43
Ethnic diversity	—	—	-8.63	-18.90*
	—	—	-.10	-.22
R^2	.71***	.65***	.71***	.68***
Number of cases	20	20	20	20

Note: In each pair, the upper figure is the unstandardized coefficient and the lower figure is the standardized coefficient.

*$p < .10$.
**$p < .05$.
***$p < .01$.

economic concentration and the size of the labor force could not be entered in the same equation. The correlation between the two variables is −.92. In contrast to Wallerstein's analysis, in which the industrial structure (operationalized as percentage of the labor force in the secondary sector) was an insignificant determinant of the level of organization, this analysis shows that the industrial structure is highly significant. Comparison of the equations does show that the equations with the labor force variable explained slightly more variation in the dependent variable. However, the differences are statistically insignificant. Thus, it is not possible to determine statistically which of the two explanations is superior. This is hardly surprising, given the high degree of correlation between the two alternative independent variables.

To explore the relation between size and unionization further, one can turn to historical data on the development of union organization. From these data, it can clearly be stated *when* size might have made a difference (see Stephens 1979: 115–116). In my earlier work, I argued that the changes after 1930 in the density of organization were largely a product of the political power of the Left. For the postwar period, this can clearly be seen by comparing the countries in which organization increased with those in which it declined. In all of the countries in which my index of left-wing government for the 1945–1970 period was less than seven, the degree of organization declined over the period; and in all countries in which the index was more than 9, it increased.[5] To examine whether membership growth for the periods 1930–70 and 1950–70 were statistically associated with the size of the labor force or economic concentration once the index of left-wing government was controlled for, regressions were run with membership growth as the dependent variable with and without ethnic diversity as an additional control variable. Since the 1930 figure for Finland was influenced by the repression of the Left in this period (Alapuro 1988) and the Italian membership figures for the early postwar period are notoriously un-

Table 3. Determinants of Union Density, 1930

Variables	Equation 1	Equation 2	Equation 3	Equation 4	Equation 5	Equation 6
Constant	74.16***	4.76	73.18***	9.47	79.86***	.18
Size of labor force (logged)	-5.01** -.52	—	-4.47** -.46	—	-4.97** -.49	—
Economic concentration	—	19.03* .41	—	19.67* .42	—	27.75** .52
Ethnic diversity	—	—	-12.99 -.24	-19.60* -.22	—	—
Linguistic diversity	—	—	—	—	-32.16** -.47	-40.65** -.59
R^2	.21*	.10	.20	.17	.38**	.41**
Number of cases	14	14	14	14	13	13

Note: In each pair, the upper figure is the unstandardized coefficient and the lower figure is the standardized coefficient.

*$p < .10$.
**$p < .05$.
***$p < .01$.

reliable, regressions were also run deleting these cases from the 1930–70 and 1950–70 analysis, respectively. In none of these 12 regressions was the coefficient for the size of the labor force or the degree of concentration significant. The highest standardized regression coefficient was −.23 ($t =$ 1.02, $p =$ .163). Most were substantially lower. In fact, in most cases, the adjusted R-squared was higher for the simple regression with left-wing government as the only independent variable than for the multiple regressions; and in no case was the R-squared from the multiple regression significantly higher, indicating that the addition of variables makes no improvement over the simple regression.

This squares with the results of Kjellberg's (1983) comparative historical analysis of the development of labor organization in 12 of the cases examined in the statistical analysis. He argues the main causes of union growth and decline after 1930 were the degree of organization and hegemonic power of the labor movement at the beginning of the period, the subsequent length of government by the left-wing parties, the strength of shop floor organization, and the development of highly centralized bargaining systems. His study also indicates that any direct influence of the industrial structure (or size) came before 1930. Wallerstein by contrast, contends that it is only late in the postwar period that the effects of size could have worked themselves out (1989, 489).

To test the hypotheses that economic concentration or labor force size had an effect on the 1930 levels of union density, regressions were run with union density in 1930 as the dependent variable.[6] The results are shown in Table 3. The analysis yields several findings of importance. First, economic concentration and labor force size both explain a significant amount of variation in the dependent variable. This is clearest in the case of the best equations, those which include linguistic diversity as a control variable.[7] Second, it is not possible to determine statistically which of these two variables is more important. The relative explanatory power of the two variables varies from one pair of equations to another.

Conclusion

The results of the quantitative analysis carried out here clearly refute Wallerstein's contention that the industrial structure is not correlated to unionization once the size of the labor force is controlled. When industrial structure is operationalized with the degree of concentration, the difference in the variation explained by the two independent variables is statistically insignificant. The further analysis was somewhat inconclusive. I have narrowed the time period in which size may have made a difference for labor organization to *before 1930.* While this appears to be contrary to Wallerstein's assumptions about the dynamics of organizing, it does not logically rule out his hypothesis. The inability to distinguish statistically between the two variables is not surprising, given the high correlation between them (−.92) and the fact that they are causally related. In this sample of countries at roughly the same level of economic development, the size of the labor force is the primary determinant of the size of the domestic market and the size of the domestic market is the primary determinant of the degree of concentration.

The impasse in the statistical analysis suggests that it is necessary to examine comparative historical evidence on the causes of cross-national differences in unionization. Kjellberg's (1983) work is an invaluable beginning in this regard. His argument is complex and it is impossible to give even a brief summary of it here.[8] While he does demonstrate the importance of union and employer centralization for the development of labor organization in the pre-1930 period, he also argues for the importance of a number of other factors such as party–union relations, movement ideology, and shop floor organization of unions. One could draw the conclusion from his analysis (though he does not do so), that the size–organization correlation is actually spurious even though no definite conclusion on this account can be supported by the evidence he presents. To mediate between these various arguments, further historical analysis of these cases would seem to be necessary.

However, in order to test Wallerstein's hypotheses against mine, it may be necessary to go beyond the set of countries selected for this analysis and compare countries in which size of the labor force and relevant features of the industrial structure do not covary so closely. While it is beyond my scope here to present an extended analysis of other countries in which industrial structure and labor force size do not covary, I should like to present a few observations along this line. Small countries in the third world represent one source of additional cases in which small size is associated with an entirely different industrial structure due to the typical features of dependent development. In these countries, the enclaves of mining and in some cases export-oriented agriculture (often initially foreign-owned), and the import-substituting domestic manufacturing sector form the center of labor organizing activity. Indeed, the levels of labor organization are typically extremely low outside these sectors. It might be objected that this is due to the authoritarian governments that have punctuated the histories of many of these countries. However, this objection would not apply to the former British colonies of the West Indies, which vary in size from slightly more than two million to under a hundred thousand. With the exceptions of Guyana and Grenada, elections with universal suffrage have been held without interruption since the 1940s; and a high degree of internal self-government was achieved by the mid-1950s, well before independence. Moreover, few governments have been overtly hostile to unions, and in the smaller islands moderate labor parties have achieved a hegemonic position. Yet union organization is much lower than it is in the smaller European countries.[9] This would appear to contradict the labor force size hypothesis and support the industrial structure hypothesis.

A second interesting comparison is that of two of the East Asian newly industrializing countries, Taiwan and Korea. For historical reasons, these countries already differed in the level of capital concentration in the early postwar period, with Korea having higher levels. More recently, conscious political decisions on the part of the Korean government led to an industrial strategy emphasizing the development of heavy industry and thus yet higher levels of economic concentration (Scitovsky 1986). This has resulted in the development of strong and militant unions based in heavy industry in Korea and relative labor quiescence in Taiwan. The Wallerstein hypothesis would predict the opposite, since the labor force in Korea is over twice as large as that in Taiwan.

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In my 1989 article on cross-national differences in unionization rates, I developed a formal model with four key assumptions, namely, that (1) unions' capacity to reduce profits in industrial conflict depends on the proportion of the relevant labor force that consists of union members, (2) the cost of organizing is an increasing function of the number of new members recruited, (3) there are costs of administering a union that are independent of the organizing effort, and (4) unions choose the level of organizing activity that maximizes the present value of the net income stream (wages minus union dues) of existing union members. Together, these assumptions imply that the optimal union density from the union's point of view is inversely related to the size of the labor force. This, I suggested, might explain the observed pattern that smaller countries generally have higher rates of unionization than larger countries among advanced industrial societies.

John Stephens presents an alternative explanation. Stephens argues that the variables I used as controls for industrial structure missed the most important way that industrial structure matters: the degree of economic concentration. Following Geoffrey Ingham (1974), Stephens argues that smaller countries tend to have more concentrated economies. This is partly because of the effect of size—small markets can only support a few firms—and partly because the small countries in Western Europen happened to industrialize late and rapidly. Late and rapid industrialization, according to Ingham and Stephens, resulted in a more concentrated economic structure. Thus, Stephens agrees, at least in part, that size matters, but he argues that it matters for a different reason. Small countries have highly concentrated industries, and highly concentrated industries are easier to unionize.

Statistical evidence cannot distinguish between the two explanations, according to Stephens, because of multicollinearity between the (log of the) size of the labor force and Pryor's (1972) data on cross-national differences in industrial concentration. The more fundamental difficulty, however, is the lack of data on industrial concentration for 9 out of 20 countries. Stephens's method for surmounting the missing data problem is to use the log of GDP to estimate the missing values. However, the effects of industrial concentration and the size of the labor force cannot be distinguished on the basis of a regression in which a nonlinear transformation of GDP is the independent variable. GDP could just as easily be a proxy for country size as for industrial concentration. Direct data on industrial concentration for more countries might reveal that the correlation between size and concentration is lower (in absolute value) than Stephens believes.

In my opinion, the weakness of the case for economic concentration stems from either logical or empirical flaws in each of the proposed links between industrial concentration and unionization. First, Stephens implicitly defines concentration in terms of average firm or plant size. Stephens suggests—and I concur—that it is easier to organize workers in one large plant than the same number of workers located in many small plants. Moreover, a variety of studies of union membership within the United States, Great Britain, and West Germany have found that union density increases with plant size.[10] Yet there is no evidence that the average plant or firm size is larger in smaller countries. Erik O. Wright's data on the distribution of workers in the private sector according to firm size reveals virtually no differences between Sweden and the United States (1985, 208–9). In fact, large firms (over five hundred employees) employ a slightly lower proportion of workers in Sweden (47%) than in the United States (51%).

Second, Stephens argues, larger countries will have more purely local or regional markets; but this has little to do with industrial concentration. The typical industries with local markets (e.g., construction, retailing, services) are not particularly concentrated.

Third, Stephens argues, a high level of concentration encourages the formation of fewer separate unions, which, in turn, encourages greater union density. The association between the number of separate unions and the size of the country is weak, however (Windmuller 1981).

Fourth, Stephens argues, highly concentrated industries are more likely to produce powerful and centralized organizations of employers. When faced with well-organized opponents, unions are forced to organize a larger share of the workforce in order to bargain effectively. This argument runs afoul of evidence from the United States and Great Britain indicating that the *less* concentrated the industry (and the *more* differentiated the product), the more likely employers' associations are to bargain on behalf of their members (Hendricks and Kahn 1982; Jackson and Sisson 1976). It is the small firms that seek the protection of a strong industry association. Large firms have the strength to bargain alone. I have argued elsewhere that centralized bargaining at the national level is related to economic openness, in particular to dependence on nonagricultural exports, rather than to economic concentration (Wallerstein 1989).

Fifth, Stephens mentions craft versus industrial unions. However, there is no direct evidence linking the type of union and union density. Nor is there any evidence that small countries are less likely to have craft unions than large countries. My unsystematic impression is that there is little relationship between country size and union type. Craft unions are strong in both Great Britain and Denmark while industrial unions dominate among blue-collar workers in both Sweden and Germany.

The most common use of concentration ratios is as an imperfect measure of market power in the product market. Although Stephens does not mention oligopolistic market structure in his list of reasons why concentration matters, there is solid evidence from the United States that unionization increases with the industry's market power.[11] The reasoning is that firms with market power have a less elastic demand for labor (employment declines less for any given wage increase), which increases the potential gains from unionization. Put another way, workers organize in oligopolistic industries in order to gain a share of the monopoly rents. It is doubtful, however, that market power can explain cross-national differences in union density, since unionization rates are highest in small, open economies. A high concentration ratio is more likely to be associated with monopoly power in a largely closed economy than in a small, trade-dependent economy with liberal trade policies.

There are large differences in unionization rates across industries. Some progress has been made in specifying what characteristics make high unionization rates more likely. Some characteristics—plant size, monopoly power—have already been mentioned. Others that are frequently cited are the capital intensity of production (although capital intensity is really an endogenous variable) and dangerous working conditions (Hirsch and Addison 1986, 60–63). The most striking difference in many countries today is that between unionization rates in the private and public sector, although 30 years ago it was the private sector that was more highly unionized.

In sum, Stephens's belief that industry characteristics are important determinants of union membership is supported by a wealth of evidence. Like Stephens, I would expect that characteristics of the economic structure are important in explaining the contrast between unionization in Western Europe and unionization in the Caribbean. I am doubtful, however, that concentration is the most important characteristic of industrial structure in explaining cross-national variation in union density. Moreover, among advanced industrial societies I suspect that the industrial structure is similar enough that most of the variance of aggregate unionization rates is due to other causes.

In the course of his argument, Stephens makes the subsidiary claim that all nonpolitical variables are less important in explaining union growth since 1930 than they are in explaining the cross-national variation that was already present in 1930. In my view, this claim must be treated with caution for two reasons. The first is that union membership and the votes received by political parties tied to unions are jointly determined. Adam Przeworski and John Sprague (1986) point to union strength as one of the most important determinants of the relative electoral success of social democratic parties. Thus, the close correlation between party and union strength reflects the help unions gave to labor parties as well as the help that labor governments gave to unions.

The second reason for caution is that Stephens loses 30% of the countries when he examines unionization rates in 1930 and 1950. Moreover, in the reduced set of countries, the correlation between social democratic governments and country size is substantially higher than in the full set of 20 countries. In other words, Stephens was forced to exclude the most useful cases for distinguishing the effect of labor governments from the effect of the size of the labor force.

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Notes

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1. This assumes that the union movement does respond by redoubling its organizing efforts. In the short run, the employers' offensive will often result in setbacks for unions. In fact, in almost all of the countries that developed centralized employers' federations, the federations' offensives did result in setbacks for the labor movement at one time or another.

2. This is also predicted by Wallerstein's second proposition that "any change that increases the marginal strike effectiveness increases union density" (p. 488).

3. Most cross-national analyses of advanced in-

dustrial democracies do not include Israel and Iceland. Statistical analyses excluding these two cases yield similar results.

4. Since addition of linguistic diversity does not improve the explanatory power of the equations, the results using all four independent variables are not shown in the table.

5. For this index a score of one was given for every year of rule by majority socialist or communist parties alone from 1945–1970. For coalition governments with centrist or rightist parties, a fraction weighted for the parliamentary strength of the Left in the coalition was given (Stephens 1979, 91).

6. In this analysis I lost six cases when compared with the analysis for the late 1970s. Thus, given the small sample, the analysis should be treated with caution. However, the sample is close to the universe of relevant cases. In Japan, Italy, and Finland the labor movements experienced varying degrees of repression. Israel did not exist. Moreover, one might question its inclusion even in the 1970s data because the role of the Histadrut and the socialist elite in the very founding of the society indicates that the dynamics of labor organizing would be significantly different in that country. Iceland might be added; but many analysts would question its inclusion because of its small size and insufficient level of industrialization. Only Ireland and Luxembourg clearly fit the criteria for inclusion in the sample and were not included because comparable data for the independent variable was unavailable. As a result, refining the statistical analysis probably would do little to resolve the question of why size makes a difference for the density of union organization.

7. Equations with both linguistic and ethnic diversity were inferior to those with linguistic diversity alone and thus are not shown.

8. An extended comparative historical analysis, including a discussion of Kjellberg's analysis, was included in an earlier version of this paper, which was presented at the 12th World Congress of Sociology, Madrid, 1990.

9. See Henry 1972 for a general discussion of Caribbean unions. Based on a 1984 survey of 11,622 respondents, Gordon found that 17.6% of workers and 29.3% of nonmanual employees in the labor force reported union membership (1987, 19).

10. See Hirsch and Addison 1986, 61–62 for a summary of U.S. and British studies up to the mid-1980s. Howell 1989 is a more recent study of U.S. data. Plant size and union density in Germany is examined by Armingeon (1988), who notes that the effect is strong in both extremes but weak for firms having more than a hundred but fewer than two thousand workers.

11. Again, see Hirsch and Addison 1986, 60–61 for a summary of the literature up to the mid-1980s. Hirsch and Berger 1984 and Belman 1988 are more recent studies.

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ON DIMENSIONALIZING ROLL CALL VOTES IN THE U.S. CONGRESS

Two related issues have developed in the scale analysis of voting in the U.S. Congress. One is methodological; it concerns the appropriate dimensionalizing model. The other is more substantive, entailing interpretation of the extent to which voting dimensions carry an ideological component. Kenneth Koford contributed to consideration of these issues in his research note, "Dimensions in Congressional Voting," in the September 1989 issue of this Review. *In this controversy, his claims are challenged vigorously by Keith T. Poole and Howard Rosenthal. In turn, Koford defends his argument that "much roll call voting in Congress does not fit a single dimension."*

Kenneth Koford has recently concluded: "Poole and Rosenthal's . . . claim to have developed a superior technique for finding dimensions remains warranted: the dimensions have substantial explanatory power. However, the dimensions, particularly the first, are not nearly as powerful as they imply" (1989, 960). This criticism is incorrect because it is based on assumptions that are not consistent with data on congressional roll call voting.

In addition, Koford presents a confused discussion of dimensional analysis and misinterprets other studies that he claims have found "ideology at best a secondary factor" (pp. 949–50). We address the dimensionality power issue first. We then comment on the proper interpretation of the result and, finally, discuss studies in which the liberal-conservative dimension (ideology) is compared to economic and other measures of constituency interests.

The Power of the First Dimension

Koford argues that the appropriate null model for dimensional studies of roll call voting is errorless spatial voting in a truly low-dimensional space—two to seven dimensions (pp. 957–58).[1] Under the null model,[2] one can compute the percentage correctly classified using a one-dimensional projection of ideal points. Since these null models show percentages in the range of 67%–75%, presumably the one-dimensional classifications[3] we obtain in the 80%–85% range are less impressive than they would be compared to a 50% benchmark.[4] This latter benchmark represents the limit of the one-dimensional classification percentage as the number of true dimensions grows indefinitely. Alternatively, the benchmark represents purely random voting.

The classical approach to a benchmark is found in Weisberg 1978. One picks a null model such as random voting, the majority, or two- or three-party models. This null model is conceptually simpler than the model being investigated. Hopefully, the model under investigation will have greater success at classification than the benchmark. The degree of improvement is evaluated by a measure of proportional reduction in error. Koford's alternative approach is radically different. He proposes adopting a more complex (because higher-dimensional) model as the null model and computing, for a hypothetical distribution of legislators and roll calls, how the model being estimated would perform if the null model

Table 1. Recovery for Simulated Voting Models

Voting Models and Dimensionality Used by NOMINATE	Geometric Mean Probability[a]	Percent Correct[b]	β[c]
Perfect voting in three dimensions[d]			
one dimension	.609–.629	70.6–72.5	19.6–21.0
two dimensions	.743–.758	83.8–85.6	28.3–31.5
three dimensions	.885–.899[e]	Computer Overflow	
Recovery of simulated one-dimensional voting with error[f]			
one dimension	.618–.635	75.5–77.0	12.7–14.3
two dimensions	.626–.638	76.0–77.7	13.7–15.1
three dimensions	.630–.642	76.7–78.2	14.5–16.1

[a] Geometric mean probability = exp(log-likelihood/(220 × 440).

[b] Percent of choices correctly classified.

[c] Signal-to-noise parameter.

[d] The entries give the lowest and highest values obtained from five simulations with 220 legislators drawn randomly through the unit circle, 220 legislators radially symmetric to these 220, and 220 roll calls whose cutting lines were defined by the 220 pairs of legislators.

[e] Geometric mean probability at last converged legislator estimates before computer overflow occurred.

[f] The entries give the lowest and highest values from five simulations with 440 legislators drawn randomly from the [−1,1] and 200 roll calls with zero midpoint.

were true. The actual performance is then compared to the hypothetical standard.

The null model of errorless voting in seven or fewer dimensions is not even remotely supported by the data. If it were correct, one would obtain *perfect* (or, to be realistic, near-perfect) classification of congressional votes with a low-dimensional Euclidean model. There is no conceptual basis for such a model. Errorless voting can always be obtained by having as many dimensions as there are legislators (or as many dimensions as there are roll calls). The number of dimensions required to achieve errorless spatial voting is an *empirical* question, not a theoretical one.

The theoretical question is, What portion of roll call voting behavior is consistent with a spatial model and what portion must be ascribed to nonspatial considerations? From a statistical standpoint, the nonspatial aspects of voting are lumped together in an "error" term. In this regard, estimating a spatial model of voting is no different from fitting any other multivariate model. All such models divide the observed behavior into deterministic and stochastic components. The equivalent of Koford's null model in the context of linear regression would be, *A perfect fit will be obtained when K independent variables are used.*

The NOMINATE and D-NOMINATE (Poole and Rosenthal 1985a, 1991) procedures are not based on the restriction to errorless spatial voting but on probabilistic voting, as suggested in the seminal paper of Hinich (1977). D-NOMINATE estimates the legislator and roll call parameters and a signal-to-noise ratio β.[5] When voting tends to perfection, β goes to infinity; and the computer analysis "blows up" and does not converge.[6] If the true dimensionality is three, for example, we will get a relatively low value of β and an imperfect fit from estimating a one-dimensional model, a higher value of β

Table 2. Recovery Using Actual Data from the House of Representatives

Dimensionality Used by D-NOMINATE	House of Representatives				
	91st	93d	95th	97th	99th
Percent Correct					
1	84.6	82.4	83.2	84.5	85.0
2	86.5	84.5	84.2	85.5	86.4
3	87.4	85.1	84.9	86.1	87.0
4	87.4	85.6	85.8	86.3	87.4
5	87.7	85.8	85.9	86.6	87.8
6	88.3	86.3	86.0	86.8	88.0
7	88.5	86.4	86.5	86.9	88.5
8	88.8	86.5	86.5	87.1	88.8
9	89.2	86.7	86.7	87.4	88.8
10	89.5	87.0	86.9	87.8	89.0
Estimated β Value					
1	13.7	12.2	13.5	12.5	13.5
2	15.8	13.3	14.8	13.8	15.3
3	17.4	14.4	15.8	14.6	16.0
4	18.1	15.0	16.4	15.7	16.7
5	18.6	15.7	17.1	16.5	17.4
6	19.3	16.6	18.0	17.1	18.2
7	19.8	17.3	18.6	17.7	19.2
8	20.5	17.6	19.0	18.5	19.6
9	21.3	18.5	20.0	19.4	20.4
10	22.3	19.4	20.5	20.4	21.0

and a somewhat better fit in two dimensions, and blowup when trying three dimensions. The upper half of Table 1 shows the results of estimating various models for ideal points uniform through the unit sphere and a uniform distribution of roll call cutting lines and cutting planes, with all of the cuts designed, as in Koford, to produce 50/50 splits. Clearly, D-NOMINATE will, by blowing up, let us know if actual data support Koford's null model.

In Table 2, we present results of running D-NOMINATE on the 91st, 93d, 95th, 97th, and 99th Houses. It can be seen, that unlike the hypothetical errorless data, D-NOMINATE does not blow up at low dimensionality. Even at seven dimensions, the upper end of Koford's null models, classifications reach only 88%. If "errorless" voting of any dimensionality is an appropriate benchmark, the benchmark dimensionality would appear to be 435, leading to a 50% criterion.

Moreover, the pattern of incremental gains to classification in D-NOMINATE discloses a highly unidimensional structure. With the exception of the second dimension, higher dimensions are basically fitting "nonspatial noise" in the data.

To see what happens in a "true" one-dimensional world with noise consider the simulation results presented in the lower half of Table 1. These simulations had a uniform distribution of ideal points in one dimension. All roll calls had expected 50/50 splits. "Noise fitting" generates gains in classifications with higher dimensions similar to those we observe with actual data. Thus, while one- and two-dimensional models do not provide perfect fits, little is gained by increasing the dimensionality.

It is important to recognize that NOMINATE does not seek to maximize classifications but to maximize a likelihood. When optimal, one-dimensional classification procedures are used (as is appropriate for Koford's models), even higher percentages are obtained (see Table 2).

In analyzing the entire two-hundred-year history of roll call voting, we (1991) have found that a second dimension improves classification only by about 3%. Koford makes a valid point in arguing that just looking at the marginal contribution of an extra dimension can understate the importance on the dimension. For example, in his Figure 3 (Koford 1989a, 953), he presents a two-dimensional example with two equally important dimensions. Since any one-dimensional projection will correctly classify 75%, the marginal contribution of the second dimension will be only the remaining 25%. However, if we project directly onto the second dimension, we also find 75% classification. In general, if two dimensions are of equal importance, separate projections will lead to equal success in classification. However, we (1991) establish that the second-dimension projection is much less successful than the first at classifying the actual roll call data. In fact, the second dimension barely betters the marginals.

To sum up, to the extent that a Euclidean analysis can capture voting in Congress, one dimension—or at most two—will do. The first dimension is very strong. Two-to-seven dimensional models of errorless voting are not an appropriate standard of comparison.

The Purpose of Dimensional Analysis

What is accomplished by Euclidean scaling? To address this question, we return to an errorless, perfectly symmetric, two-dimensional world. Legislator ideal points might be uniformly distributed on the circumference of a circle, as in Koford's Figure 3 or, more realistically, uniformly distributed throughout the circle. Suppose, following Koford, that all roll call votes could be represented as cutting lines through the center of the circle, generating even, 50/50 splits. In such a world, any cut through the circle will do equally well as a classifying dimension (75%). On the other hand, if ideal points become correlated, so that the distribution is ellipsoidal, the major axis of the ellipse will do better than any other cut. As the circle gets squashed into a cigar shape and then into a line, the major axis will improve to classifying 100%. Similarly, if roll call cutting lines are not uniformly distributed, one axis will do best at classifying.

Roll call scaling thus presents strong similarities to eigenvector extraction. In that procedure, the first eigenvector is chosen to "explain" as much variance as possible and so on. The pattern of eigenvalues and eigenvectors discloses important aspects of the structure of the data.

Roughly speaking, dimensional analysis reveals the extent to which the world is cigar-shaped, for whatever reason. When either roll call alternatives or legislators are concentrated along a single axis, a one-dimensional model will be excellent at classification. In our two-dimensional example, the major axis will be much more important than the dimension perpendicular to it.

Of course, axes can always be rotated to create dimensions of more nearly equal importance. If, instead of using the major and minor axes of the ellipse, we used a 45-degree rotation of the axes, we would find two dimensions that classified equally well. But using this rotation would obscure the key point—we are looking at a highly squashed ellipse, not a circle.

Koford simply misses this point in his example of seven voters and two roll calls (p. 950). In that example, the ideal point distribution is correlated so that in essence, the major axis of the two-dimensional ellipse would be on the 45-degree line. A one-dimensional analysis of the votes, rather than recovering either of the two axes Koford plots, would array voters along the major axis and, as Koford points out, successfully classify 12 out of 14 votes. A two-dimensional analysis would add an axis perpendicular to this and, in addition, recover roll call cutting lines that correspond to Koford's axes.

In other words, scaling would disclose all the important features of the example. The first dimension is "more important" than the second because ideal points are correlated. However, as evidenced by the roll call cutting lines, the two "issues" voted on are perpendicular to one another.

Koford's discussion, in contrast, confounds the correlation of ideal points in his example with the orthogonality of the two roll call votes in the same example. The discussion becomes hopelessly confused: "Votes are perfectly estimated, . . . implying three dimensions. But regression can overstate the number of dimensions" (p. 950)—indeed, since with only two roll calls, it is well known that a two-dimensional model is guaranteed to provide a perfect fit to the data! Koford goes on to state, "Dimensional analysis also fails to fit a second dimension properly if it is fitted to the residuals from the first dimension" (p. 950). But our roll call methods are not based on fitting residuals.

Our recovery of legislator positions, we should point out, is *not*, given large enough samples, dependent on the distribution of roll call votes. Suppose, for example, we had αN orthogonal roll call votes $(0 < \alpha < 1)$, as in Koford's Figure 1 and $(1 - \alpha)N$ roll call votes uniformly distributed about a circle, as in his Figure 3. Then, for N sufficiently large, our recovery will essentially be independent of the value of α. In all cases in this example, our first dimension would be the major axis of the legislator ideal point distribution.[7] Presumably, for $\alpha = 1$ Koford wants to define the dimensions on the basis of the orthogonal roll call cutting lines. But how would he define the dimensions when $\alpha = 0$ and roll calls are symmetrically distributed? The natural route to go is the conventional one of maximizing some criterion—explained variance, classifications, or likelihood. When this is done, we find a powerful first dimension.

Koford rightfully emphasizes that unidimensionality can result from several different sources—cognitive limitations, restrictions on the agenda, coalitions, and logrolls. This is true. We have not provided a structural model of congressional behavior but have uncovered what we believe is an important aspect of the "reduced form" of the political process.[8] As Koford points out, some earlier research found higher dimensionality simply by imposing it a priori. Other research found higher dimensionality because it used techniques not designed to recover Euclidean voting (see Morrison 1972 for a critical analysis).

Ideology and Constituency Interests

Finally, we take up the claim that "ideology [is] at best a secondary factor" (Koford 1989a) in studies that consider constituency interests. One can operationalize ideology as Americans for Democratic Action (ADA) ratings or our scaled estimates on the first dimension. Ideology and constituency characteristics are likely to be collinear. As such, any conclusions are likely to be hostage to the problem of marginal evaluation that Koford raised in regard to scaling. We addressed this issue some years ago (1985c) in an article not discussed by Koford. We briefly summarize the findings.

Using NOMINATE, we estimated β and the spatial coordinates for the Senate in

1977. We used 100 senators and 568 roll calls with more than 2.5% voting in the minority. This means we estimated 1,238 parameters. We correctly classified 82.3% of the votes. For the same roll calls, we then ran a linear logit using as variables party, income, growth, education, urbanization, union membership, age, and manufacturing. This set is similar to that used in Peltzman (1984), cited by Koford. Separate coefficients were estimated for each roll call, leading to a total of 5,680. Nonetheless, classification was improved only to 82.8%, despite the more than quadrupling of parameters. We next added an ideological residual as a regressor. For senators serving prior to 1977, we used their 1976 coordinates (Poole and Daniels 1985). For the remaining senators, we used their 1978 coordinates. Thus, we used only out of sample information. These coordinates were then regressed on the "constituency" variables. The regression residual was entered in the linear logit. This raised classification by 3.4% to 86.2%. So even attributing as much as we can to constituency variables, our first dimension adds (slightly) more to classification than adding a second dimension to a one-dimensional spatial model. As a stand-alone model, the far more parsimonious ideological model does almost as well as a constituency interest model. These results show that ideology is not a secondary factor.

Moreover, we applied the same strategy to the 10 strip-mining roll calls analyzed by Kalt and Zupan (1984), another study cited by Koford. Here we estimated the linear logit model using the seven issue-specific constituency variables chosen by Kalt and Zupan. Our finding was reinforced. In fact, the log-likelihood for the linear constituency model is actually less than that for the more parsimonious NOMINATE model. Again introducing the residual leads to significant improvement. Our findings confirm and generalize Kalt and Zupan (1984, 291), who comment: "The striking finding is how well voting on an issue with as much pocketbook content as [strip mining] can be explained by senators' positions on the death penalty, sex education, the neutron bomb, the ceding of the Panama Canal, the immigration of avowed communists, and so on." In other words, a single dimension relates to a wide range of issues. In their concluding sentence Kalt and Zupan write, "For now, it appears that the economic theory of regulation will have to keep the door open to ideological behavior" (1984, 298). Clearly, in contradiction to Koford's assertion, these authors do not view ideology as a "secondary factor."

While we do not deny that constituency-specific factors affect some individual decisions on some roll calls, we find that Koford has simply misread the evidence that results when one compares the constituency studies to the ideological alternative. In addition, we have pointed out that the quantitative assessments in his article are based on an inappropriate benchmark. To the extent that congressional voting can be described by a spatial model, a unidimensional model is largely (albeit not entirely) sufficient.

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This controversy is largely about how to determine the "true" number of dimensions in roll call data. In my original article I questioned Poole and Rosenthal's claim of high "prediction success" for a single dimension by pointing out that alternative, null hypotheses also did well. A single dimension was able to correctly classify only around 30%–50% of the roll call votes that would not also be classified correctly by two-to-four-dimensional spaces.

Poole and Rosenthal make two points. First, they respond to my (1989a, 1990) analysis of their dimensional findings—that while there is an important first dimension in congressional roll-call voting, they overstate its importance. They restate their original analysis and describe new work that advances their analysis of legislative roll calls. They also admit my main claims. In particular, they have examined more complex null hypotheses (1991; Poole, Sowell, and Spear N.d.) with findings similar to mine.

Second, they criticize Peltzman and others who claim that roll call votes are based upon specific economic interests rather than ideology. In so doing, they misstate Kalt and Zupan's views and misquote me; while I have some sympathy for Peltzman's view, I have argued that it *overstates* the dimensionality of roll calls much as Poole and Rosenthal *understate* it.

I begin with a look at Poole and Rosenthal's new arguments for the unidimensional model and its extension to as many as 50 dimensions. Their new work leaves a puzzle: How can a high proportion of roll call votes fail to fit even high-dimension models? I show why their method cannot find dimensions on which legislators place differing importance. Finally, I show that econometric studies of roll call voting find that personal ideology is an important secondary factor after constituency. However, these studies miss the true degree of unidimensionality by omitting its effects when correlated with constituency variables. Analysis of roll call votes need a theory of the sources of legislators' choices, which is still absent from Poole and Rosenthal's work. The unidimensional findings could be due to constituency preferences, the effect of the two-party system in constituencies, congressional coalitions, or legislators' genuinely ideological feelings. Available data gives some hints favoring the party system in constituencies and suggests additional tests.

What Is a Proper Null Hypothesis?

The purpose of a *null* hypothesis is to identify a standpoint from which to judge the statistical success of a particular theory. For example, in econometric analysis, an R-squared of zero is the null hypothesis that no independent variables are statistically significant. Poole and Rosenthal confuse a null hypothesis with another approach, comparing the relative merits of two *alternative* hypotheses. But there are no alternative theories to compare their results against.[9]

An appropriate null hypothesis can be defined in several ways. Poole and Rosenthal prefer the hypothesis of an infinite number of equally important dimensions.[10] That is a very remote alternative to their original hypothesis of a single dimension that explains all voting. Parsimony suggests a small number of dimensions (such as two) to compare with a one-dimensional model. Are the results sensitive to increasing the number of dimensions beyond two? Using moderately different assumptions, several researchers have examined this question. The proportion of votes correctly classified falls, but Poole and Rosenthal (1991) and Snyder (N.d.) obtain correct classification percentages close to mine for the null hypothesis of a moderate number of equally important dimensions fitted to a single dimension (see Table 3)[11] (I calculated two percentages. They differ by the weighting assumption in the *n*-dimensional space. The first assumes that roll calls occur at vertices, which means in high-dimensional spaces that most roll calls are "close" to the assumed cleavage. The second assumes an equal number of roll-calls on each dimension.)

Thus, my original small (two-to-four) dimension null hypothesis of 67% is conservative compared to other scholars, including Poole and Rosenthal. My alternative null hypothesis of 75% is close to their average.

In recent work, Poole and Rosenthal

Table 3. Correct Classification Percentage by Number of Dimensions

Number of Dimensions	Koford 1989a	Koford 1989a	Poole & Rosenthal 1991	Snyder N.d.
2	.75	.75	.789	.85
4	.625	.75	.732	.787
7	.588	.719	.699	.696

have considered large numbers of dimensions. For this work, a high-dimension null hypothesis is reasonable. Poole and Rosenthal (1991) propose a 65.9% null hypothesis representing an infinite-dimensional space with the empirical average majority for Congress. Their comment proposes a 50% null hypothesis. Yet whatever the null criterion chosen, the bottom line is the same. A large share of roll call voting remains unexplained by the dimensional model.

The proportion of roll call voting not explained by Poole and Rosenthal's (1985b) House estimates is given below, for the various null hypotheses just described. Those estimates correctly classified 84.03% of the roll call votes. For the three low-dimensional null hypotheses, the average of correct classification for two, four, and seven dimensions are used.

Koford 1989a	45.6%
Koford 1989a	61.3%
Poole and Rosenthal 1991	61.4%
Snyder N.d.	75.7%

Alternative, and infinite-dimension null hypothesis, such as Poole and Rosenthal's two proposals, could be used. In my original article I proposed that dimensions could vary in importance by a constant ratio, here chosen to be 1.5:1.

Koford 1989a	45.4%
Poole and Rosenthal 1991	46.8%
Poole and Rosenthal comment	31.9%

When Poole and Rosenthal extended their model to fit large numbers of dimensions I anticipated that the additional dimensions would fit most unexplained voting. But Poole and Rosenthal (above and 1991) try to fit up to 50 additional dimensions to the roll call data and find that they cannot fit most of the remaining variation. Their Table 2 fits dimensions to House voting for six Congresses; average correct classification for one dimension is 83.9%, while for 10 dimensions it rises to 88.04%. It is striking how much of the voting *does not fit even 10 dimensions* and also how little fits the added 9 dimensions.[12] Dimensional analysis' biggest theoretical problem now, it seems to me, is to explain how such a large share of roll call votes can fail to fit so many dimensions.

One possible explanation is that these votes are just "mistakes." Legislators are busy, sometimes fail to identify their true interests, and err. However, the proportion of "noise" votes is too high for this explanation to be plausible. If legislators who choose randomly are correct half of the time, the true proportion of randomly made votes is *twice* the number that are

Table 4. Explanatory Power of One and Many Dimensions (%)

Votes Explained by	Koford 1989a	Poole & Rosenthal 50%	Poole & Rosenthal 65.9%
1 Dimension	54.3	67.8	52.8
2–10 Dimensions	11.7	8.3	12.1
Unexplained	34.0	23.9	35.1

not correctly classified. For Poole and Rosenthal's proportion correctly classified by one dimension of 84.03%, 15.97% of the votes are "errors" and 31.94% of all votes are cast randomly. If randomly choosing voters vote for a bill in the same proportion as the average voter, 55% of votes will be correct and 35.5% of all votes are "random."[13] That seems too high to be credible.

Is "Noise" Voting Really Individual Differences in Intensity?

It is puzzling that adding dimensions to the unidimensional model does so poorly. It makes sense to view issue choices as locations in a dimensional space. So Poole and Rosenthal's basic approach seems correct. Rather, the failure could be due to an inappropriate assumption. Specifically, some legislators care a lot about some dimensions, while other legislators care a lot about other dimensions. But Poole and Rosenthal's unfolding model assumes that all legislators have circular indifference curves and thus identical relative intensities of preference.

Yet in legislative politics legislators are often concerned with interests of special concern to a few legislators (Lowi 1964; Mayhew 1966). The economic theory of politics shows that Lowi's "distributive" and "regulative" issues imply differences in legislators' intensity of preferences across issues (Becker 1983; Peltzman 1976). Van Schurr (1987) has found that multidimensional models fail to fit some European party data due to differences in the intensity of concern for certain dimensions. The puzzle of why additional dimensions fail to fit roll call data could be explained by this difference in intensity.

Figure 1 shows the basic point with a simple example. Poole and Rosenthal's method assumes that all legislators have circular indifference curves.[14] In Figure 1 all legislators care equally about the horizontal dimension but vary in the intensity of their preferences on the vertical dimension. Legislator I values the two dimensions equally, Legislator II considers the horizontal dimension three times as important as the vertical dimension, and Legislator III values the vertical dimension at three times the horizontal dimension.

Legislator I always prefers the alternative closer to I's ideal point, but that is not true of either Legislator II or III. Comparing points A and B, A is closer to II's ideal point, but II prefers B. A is also closer to III's ideal point, but III also prefers B. No renormalization of the space can eliminate this problem.

We can now see how well the best one-and-two-dimensional fits will classify legislators who have different relative intensities of preferences in a two-dimensional basic space. There are three types of legislators, as just described. To simplify, all three types are assumed to be uniformly distributed over a circle (as in Poole and Rosenthal's comment, Table 1).

The example is illustrated in Figure 2. Type I voters care equally about both dimensions, while Type II voters care most about the horizontal dimension, and Type III voters care most about the vertical dimension. Roll call votes occur all around the circle, arranged so that they divide the voters equally. An example in Figure 2 is proposal P, opposed to the alternative P′.[15] Correct classification of these roll calls is examined for *two* dimensions and for a single dimension. It turns out that *no* dimensional framework produces perfect classification, even though the model fitted has as many dimensions as the data.

With the alternative proposals P and P′ on the floor, the best-fitting cleavage is CC′. That fits the Type I legislators perfectly. Type II voters have the cleavage II-II′, while Type III voters have the cleavage III–III′. These cleavages are chosen to assure that points on II–II′ are equidistant between P and P′, weighting the horizontal direction at twice the vertical direction, and that the points on III–III′ are equidistant between P and P′ while weighting the vertical direction at twice the horizontal direction (for the method, see the Appendix). The result is that the Type II voters in the lightly shaded area are correctly classified, and the Type III voters in the darkly shaded area are incorrectly classified. Clearly, no adjustment of the cleavage CC′ can improve the fit. This example shows that adding more dimensions will not produce a perfect fit when not all voters have the same relative intensity.

Calculations for any specific relative intensity of preference *r* may be made with this approach (see the Appendix). The proportion of votes correctly classified are presented in Table 5. This evidence indicates that a two-dimensional model will not fit perfectly with two-dimensional data when voters differ in their relative intensity across dimensions. The fit becomes worse as the relative intensity increases. If there were not Type I voters who valued the two dimensions equally in this example, correct classification would approach .75 as a limit. While only a two-dimensional case has been considered, the principle behind this case implies that higher-dimensional models will also fail to fit this data and that data with a higher-dimensional structure and different intensities across the different dimensions will fail as well. As the number of dimensions rises and the distribution of intensities

Figure 1. Legislators with Varying Relative Intensities of Preference

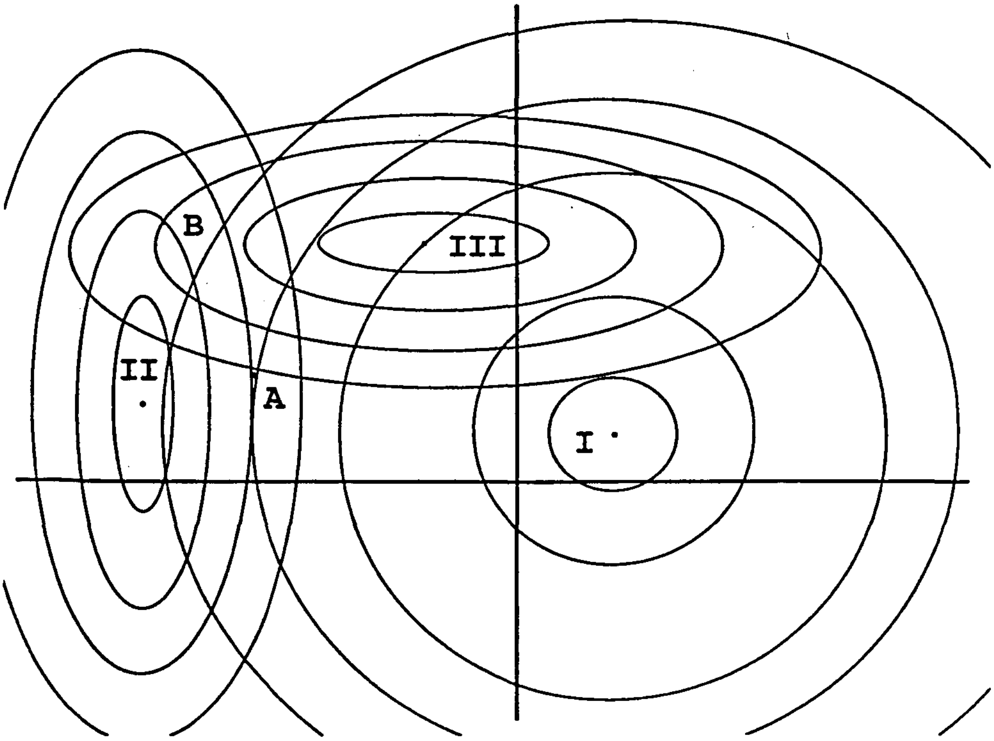
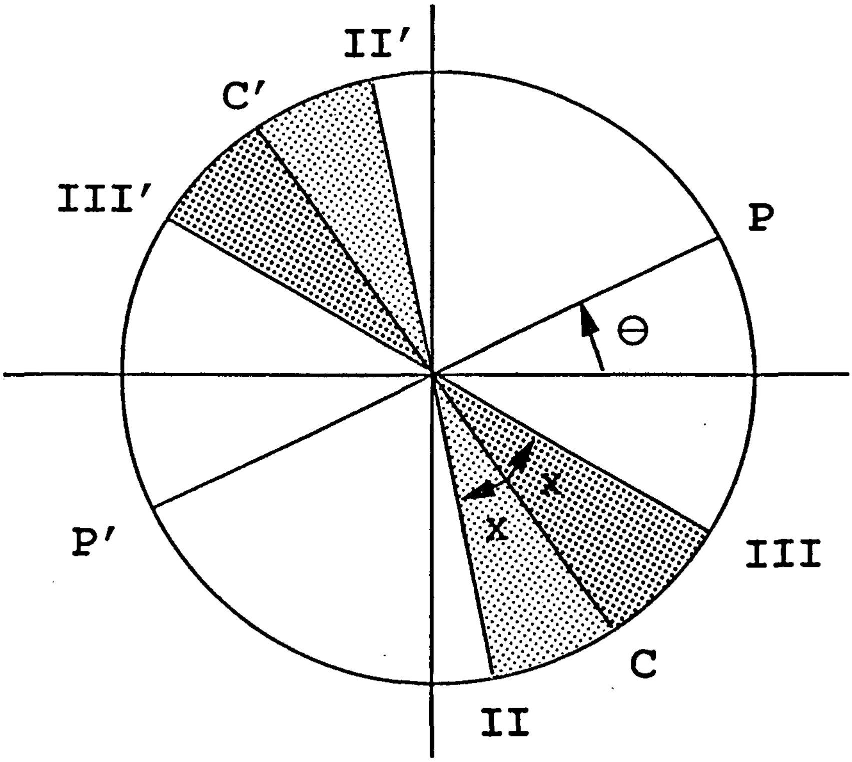


Table 5. Correct Classification by Relative Intensity

Relative Intensity	Unidimensional Correct Classification	Two-Dimensional Correct Classification
1:1	.75	1.00
1.414:1	.75	.958
2:1	.775	.915
3:1	.800	.882
4:1	.812	.867
5:1	.820	.861
∞:1	.833	.833

Figure 2. Optimal Cleavage with Three Different Relative Intensities of Preference in Two Dimensions



becomes more complex, fitting the data should become more complex as well.

Given the theoretical importance of differences in the relative importance of issues across legislators, it would be desirable to examine their actual distribution. How great are the differences in issue importance across legislators? How do they vary across issues? In addition, it is likely that the locations of legislators may determine their relative intensities. Do outliers on intensity tend to have "high" demand in terms of location, as one might predict of cotton or peanut bills? Effort could be put into identifying the locations in the issue space of legislators with high *gradients* on the issue.[16]

Poole and Rosenthal's results seem most consistent with one important dimension on which all legislators have similar intensity of preference and a number of other dimensions on which relative intensity varies. They successfully fit the first dimension; but their method fails for the other dimensions, creating "noise."[17]

Empirical Studies of the Importance of Constituency and Ideology

Poole and Rosenthal quote me out of context twice as claiming that "ideology [is] at best a secondary factor" (pp. 955, 959). In fact, I described the conclusion of most econometric roll call voting studies, as the context makes clear. McArthur and Marks state the same view: "Recent contributions to the empirical literature on legislative voting start from the premise that constituent interests are systematic determinants of voting behavior. In order to get reelected, legislators must further the pocketbook interests and ideological views of their constituents. Whether legislators also vote partly on the basis of their own ideologies is the subject of some controversy" (1988, 461). For them, constituency variables largely determine roll call votes, while ideology is "at best a secondary factor." Similarly, Peltzman states:

> The tendency for legislators to shirk serving their constituents' interests in favor of their own preferences (ideology) seems more apparent than real. Ideology measures can explain much legislative voting behavior statistically. But they turn out to be proxies for something more fundamental: liberals and conservatives tend to appeal to voters with systematically different incomes, education, and occupations, and to draw contributions from different interest groups. And these systematic differences prove, by and large, capable of rationalizing voting patterns without much need for relying on explanations that involve shirking. (1984, 210)

Richardson and Munger state, "Our results indicate that far from shirking, representatives painstakingly voted in the interests of their constituencies (1990, 12).

Numerous studies of roll call votes first estimate the importance of constituency variables and then evaluate the significance of ideology. Table 6 lists these studies and their description of the ideology variable.[18] Mueller's (1989, 213) review of this literature concludes that both economic and ideological factors matter. Weingast and Marshall find that "all studies provide substantial evidence that [constituent interest] systematically—though not necessarily completely—affects congressional voting," supporting their Assumption 1, namely, "Congressmen represent the (politically responsive) interests located within their district" (1988, 137). In contrast, they describe the possibility of "ideological voting" as controversial.

Kalt and Zupan (1984, 1990) find ideology to be of great importance in their analysis of roll call voting on energy issues. They originally expected that constituency variables would dominate, but ideological measures were equally successful in explaining their roll calls, as Poole and Rosenthal (p. 960) state. Never-

Table 6. Econometric Estimates of Ideology in Roll Call Voting

Study	Year	Issue	Description of Ideology	Statistical Importance of Ideology
Abrams	1977	NOW accounts	Party: logrolling or philosophy	Most important
Chappell	1981	Cargo preference	Ideology	Marginal
Chapell	1982	Seven issues	Ideology	Very significant; importance not stated
Coughlin	1985	Domestic content	Personal ideology	Unimportant
Danielsen & Rubin	1977	Decontrol of crude oil, 1973–74	Party: probusiness philosophy	Important
Davis & Porter	1989	Coal strip-mining	Personal ideological consumption	29% of total variation
Dougan & Munger	1989	Senate voting	Shirking	Not found
Jackson	1974	61 votes on varied issues	Trusteeship; Coalitions	Not important; Very important
Kalt	1981	36 crude oil price votes, 1973–77	Public interest goals	Important
Kalt & Zupan	1984	Coal strip-mining	Personal ideological consumption	43% of total variation
Kau & Rubin	1978	Minimum wage	Ideology or coalition	Very significant; importance not stated
Kau & Rubin	1979	26 votes on varied issues	Ideology	Significant; importance not stated
Kau, Keenen & Rubin	1982	Eight bills on economic regulation	Constituent ideology	Very significant; appears important
Ladha	1984	Coal strip-mining, 10 roll calls, 1977	Ideology	Very significant; very important
Langbein & Lotwis	1990	Gun control	Ideology	Very significant; important
McArthur & Marks	1988	Domestic content of automobiles	Personal ideology	Modest
Nelson & Silberberg	1987	National defense	Shirking	Sometimes significant; importance not stated
Nollen & Iglarsh	1990	International trade	Party and region	Most important
Peltzman	1984	331 votes on many issues	Ideology independent of constituency	8–11% of variance
Peltzman	1985	835 votes on economic issues	"Inertial" voting similar to ideology; Unidimensional economic interests	Important; Very important
Richardson & Munger	1990	Social security	Constituent values	Very significant; usually important
Vesenka	1989	Agricultural issues	Shirking	Important

theless, Poole and Rosenthal take Kalt and Zupan's quote out of context. Kalt and Zupan's *full* last paragraph reads:

> If the concept of ideological shirking does prove to be significant, its usefulness will depend on the development of models that can predict the conditions (for example, types of issues, institutional settings, economic contexts) under which ideological shirking is likely to be an important phenomenon. Of course, *it still may be that the phenomenon does not even exist. There may yet be constituent interests missing from this and previous analysis that will explain away ideology's importance in specific-issue politics.* The search for these interests should continue. For now, it appears that the economic theory of regulation will have to keep the door open to ideological behavior. (1984, 298; emphasis mine)

Thus, Poole and Rosenthal distort Kalt and Zupan's (1984) view that ideological shirking may not exist but just reflects imperfect constituency measures and that ideology is a *residual* that measures failure to vote for one's constituency. Thus, statements that "ideology matters" in the econometric studies are very different from Poole and Rosenthal's unidimensional findings.[19]

In contrast, I claimed that "regression estimates [have] biases that overstate the number of dimensions. Overall, fewer dimensions are found than seem consistent with the wide variety of constituents' preferences on issues" (1990, 59). I concluded, "The unidimensional model's success is greater than would be expected if voting represented constituencies" (1989a, 960). My view has been very different from the view Poole and Rosenthal attribute to me and more favorable to their work.

The basic difference between the econometric and dimensional approaches is that they ask different questions. The econometric studies try to explain *individual* legislators' behavior. They use the principal–agent model to see whether legislators are loyal agents of their constituents or whether they shirk and pursue their personal political agendas. In contrast, dimensional studies examine *aggregate* behavior. They try to explain the overall pattern of roll call votes. That pattern could be due to the distribution of constituency preferences, to party cohesion, or to personal or constituency ideology. Since the overall pattern of voting determines outcomes, the dimensional approach has an essential key to explaining legislative politics.

The personal ideology found in econometric studies could have been idiosyncratic, with highly personal tastes on many dimensions; but it is not. Rather, Kalt and Zupan (1990) find that legislators' personal ideology is a choice to move along the left–right ideological dimension. Poole and Rosenthal (and Poole and Daniels 1985) have shown that this left–right ideology dimension is very prominent and essentially the same as their first dimension. Whether it represents 35%–45% of roll call votes (as I conclude) or 50%–70% (as Poole and Rosenthal prefer), it is a crucial causal factor in legislative politics. Yet its source remains unclear. I have suggested (1990) that it is due to natural legislative coalitions serving the function of logrolling (as Jackson [1974] and Kau and Rubin [1979] previously argued). Or voters may use the liberal–conservative dimension to conceptualize candidates' political positions. Alternatively, Peltzman (1985) may be correct in arguing that this main dimension represents pure constituency voting based on redistributive economic issues. Some hints regarding which answer is correct are given by past studies.

"Personal ideological shirking" occurs along the liberal–conservative ideological dimension, which is also Poole and Rosenthal's major dimension. Several theories of unidimensionality imply a pressure toward unidimensional voting. First, the two-party system in each constituency could force legislators' ideal points toward the ideological dimension orthogonal to the party cleavage. This im-

Table 7. Ideology Residuals of Senators in Quartiles in Order of Estimated Conservative Ideology

Most Conservative		Moderate Conservative		Moderate Liberal		Most Liberal	
Number	Residual	Number	Residual	Number	Residual	Number	Residual
1–5	2.97	26–30	1.02	51–55	.39	76–80	-1.03
6–10	2.87	31–35	.37	56–60	.20	81–85	-1.38
11–15	1.06	36–40	-.93	61–65	1.76	86–90	-1.83
16–20	2.54	41–45	-1.84	66–70	.56	91–95	-1.99
21–25	.17	46–50	.35	71–75	-1.38	96–100	-1.85
Average	1.83		-.21		.31		-1.62

Note: Positive residuals = more liberal than predicted; negative residuals = more conservative than predicted.

Source: Residuals from Kalt and Zupan 1990; liberal ideology from Zupan, personal communication, 6 Nov. 1990.

plies a strong national influence on district cleavages, which could be party ideology, party control, or special interests. Since the influence has existed for at least a century, it is not a recent influence like political action committees. This influence must vary across districts, since "personal shirking" varies across districts. This theory implies that the unidimensional residual could be explained by the two separate party constituencies, including elites, contributors, and activists (Fiorina 1974; Peltzman 1984).

This explanation implies that unidimensionality is caused by the fundamental differences between the parties' constituencies. The party cleavage should center on the most prominent difference among constituents—perhaps to minimize transactions costs (Koford 1990). The conflict between rich and poor over redistributive taxing and spending is the obvious difference (Peltzman 1985).

A second explanation is internal coalitions within the legislature. The parties (and related coalitions) take opposing locations in the issue space. Legislators join these coalitions, moving their *identified* ideal points closer to the coalition ideal points. While the individual legislators' preferences are not unidimensional, the coalitions' observed voting positions will be unidimensional.

The coalition ideal points tend to be located between the centers of the two coalitions.[20] This theory implies that relatively *extreme* legislators will tend to move toward the center to join coalitions. So the "personal ideological preferences" of extreme legislators should be more moderate than their constituency-based ideal points. In contrast, the "different constituents" explanation makes no such prediction but, rather, implies that errors should be random. This test is shown in Table 7 for the 1977–78 U.S. Senate. It compares predicted ADA liberalism ratings (Zupan, personal communication, 6 Nov. 1990) with the "ideology residual" (Kalt and Zupan 1990, Table 2). The data support the coalitional hypothesis.[21] Senators who are predicted to be relatively "extreme" turn out to be considerably less extreme in fact. The most reasonable explanation for this pattern is that the ideal points of relatively extreme legislators are pushed toward the center by some omitted factor. And the most likely influence is the process of establishing coalitions in the legislature.

Another possible explanation is that proposed by Peltzman (1984), namely, that constituency preferences are fundamentally mismeasured, by neglecting the fact that reelection constituencies are skewed along party lines. This difference in constituencies must be fully modeled empirically to determine whether ideology really is an influence—or merely omitted constituency factors. For example, interaction terms between party and constituency may improve the specification. Peltzman's (1984) measure of party-related individual constituency differences (based on county-level data) was creative but not a full measure. Fiorina (1974) gives a complete theoretical model of these party-related differences in constituent voting, elite influences, and activist preferences. Peltzman (1985) argues that the ideological dimension should be due to a single economic variable, which he finds is economic redistribution between rich and poor. He finds strong evidence of a large redistributive dimension in economic roll call voting since 1910.

Peltzman has shown that the numerous econometric studies of Table 5 could be omitting party-related factors that cause unidimensionality. The "personal ideology" variables may well reflect the absence of correctly specified constituency-based party variables. However, the alternative hypothesis of party cohesion or logrolling needs to be fully tested against this hypothesis.[22] Empirical tests of these alternatives were developed by Jackson (1974) and include such variables as committee membership, the president's announced position, and the committee leadership position. With the superior constituency data developed in such studies as Kalt and Zupan (1984), this alternative hypothesis could be considered.

Nevertheless, Peltzman's approach suggests that the constituency and unidimensional results can be reconciled. Econometric studies have found constituency variables that explained voting but have made little effort to determine whether the independent variables as a whole could be a single "factor" like the rich-poor redistributive conflict that Peltzman describes. The unidimensional findings may be explained by such redistributive politics.

The remaining issues have yet to be explained. Such issues as abortion, civil rights, and many local distributive and regulative issues do not fit the single dimension (as Poole and Rosenthal point out [1985a, 373]). They presumably fit *some* dimensional structure. Such redistributive issues as abortion and civil rights should fit Poole and Rosenthal's model as additional dimensions. Distributive and regulative issues appear to require a modified fitting method to deal with the variation in intensity of preferences (as discussed earlier) before dimensions can be found for these issues.

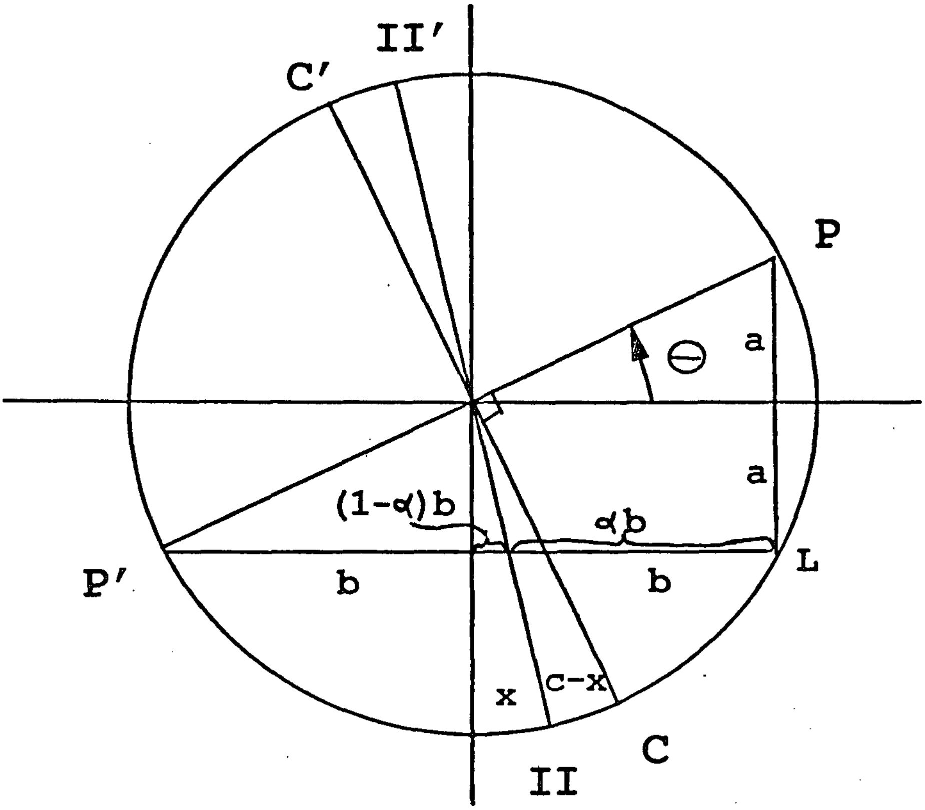
Conclusion

In my original article I showed that much roll call voting in Congress does not fit a single dimension, a point now supported by other studies (Poole and Rosenthal 1991; Poole, Sowell, and Spear N.d.; Snyder N.d.). These studies show that roll call votes that are high-dimensional nevertheless give high levels of correct classification for a single dimension. Thus, such levels of correct classification cannot be taken as evidence of unidimensionality.

Much roll call voting remains unexplained by a single dimension. Yet Poole and Rosenthal have failed to explain it by additional dimensions. I have shown that it may be caused by the incorrect modeling assumption that legislators have the same relative intensity of preference for all issue dimensions. This problem must be solved to learn the "true" number of dimensions and their relative importance.

Most econometric studies have found a significant ideology dimension; its cause

Figure A-1. Calculation of Correct Classification with Varying Intensity of Preferences



is unclear. It may be due to the omission of variables based on the party cleavage in each constituency (as Peltzman has argued) or to the effect of coalition building and logrolling in the legislature.

Appendix: Optimal Cleavages for Voters with Varying Intensity of Preferences

Voter Types II and III have cleavages symmetrically around the Type I equal-intensity voters' cleavage. In Figure A-1, which replicates Figure 2, the angle from the horizontal axis Θ defines the proposals P and P′. For these proposals the Type I voters will divide on the CC′ cleavage that is equidistant between P and P′. CC′ represents the optimal two-dimensional classification, as noted in the text. Now, the Type II voters' cleavage II-II′ will cross the horizontal line P′L where the distance

$$[(2a)^2 + (\alpha rb)^2]^{1/2} = (1 - \alpha)rb + rb.$$

Solving for α gives

$$\alpha = 1 - (1/r^2)(a^2/b^2).$$

Finding Angle X in the figure will give all of the required information. Now, tan X

$= (1 - \alpha)b/a$; and substituting for α, $\tan X = (1/r^2)(a/b)$. Since, from the figure, $\tan \Theta = a/b$, so $\tan X = \tan \Theta/r^2$. With this formula, one can calculate X for any r and Θ. The calculations used angles at 5-degree intervals. The proportional error is then $(\Theta - X)/180$. Given the symmetry, the Type III voters have exactly the same correct classification as the Type II voters.

The calculation of error in the unidimensional case takes the chosen dimension as the horizontal dimension, so the unidimensional cleavage is vertical. Any cleavage will do as well. Type I legislators are identical to those I considered in my original article and so have a correct classification of 75%. For the Type II legislators, Angle X gives the incorrectly classified voters and $X/180$ the proportion incorrectly classified. For type III legislators, Angle $\Theta + (\Theta - X)$ gives the number incorrectly classified, so long as this angle is less than 90 degrees. For Type II and III voters together, then, the proportion incorrectly classified is $\Theta/180$, for Θ such that $2\Theta - X < 90$. For Θ s.t. $2\Theta - X > 90$, a higher correct classification is obtained by reversing the assumed liberal and conservative directions, and then the number incorrectly classified is $180 - 2\Theta + X$.

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Notes

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1. Koford does not support this view by addressing roll call voting directly; instead he uses a content analysis of energy bills.

2. Assumptions about the distribution of legislator ideal points and roll call cutting lines are required. Koford places the legislators at vertices on the *n*-dimensional hypercube. This seems unrealistic, as it makes all legislators extremists and allows for none of the "moderates" that are commonly believed to exist in Congress. Koford also assumes a 50/50 vote division on every roll call. Again, this is unrealistic. Better null models can be computed by assuming a uniform distribution of ideal points over the unit hypersphere and a radially symmetric distribution of marginals in Congress. See Poole and Rosenthal (1991).

3. To save space, we focus the discussion on the NOMINATE procedure we developed and its multidimensional, dynamic extension, D-NOMINATE (1985a, 1991). Koford also discusses the work of Poole (1981) and Poole and Daniels (1985). The NOMINATE procedure is superior to the earlier work, since it uses all roll call votes directly, whereas the earlier work scaled interest group ratings that were constructed from roll call votes. However, the two procedures cross-validate. Therefore, the spirit of our discussion applies to both methods.

4. The 50% standard is appropriate only when, as Koford assumes throughout, all roll calls are even splits. Otherwise, the average percentage voting on the majority side is the correct benchmark. In Poole and Rosenthal 1985b (cited by Koford), we show that NOMINATE does better than 80% in one dimension, even on close roll calls with less than 60/40 margins, while we elsewhere (1987) show that NOMINATE outperforms the majority model.

5. We estimate an additional parameter, *w* (1985a). In later work, we have fixed *w* at 1/2.

6. In other words, like standard probit and logit methods, D-NOMINATE blows up when the model achieves perfect classification.

7. More generally, the placement of the first dimension will change as the distribution of roll calls changes. Assume, however, that the data is generated by a true *n*-dimensional world with error as specified in the D-NOMINATE model. Then, as long as the sample of roll calls gives sufficient coverage of the space, the interpoint distances between legislators recovered by D-NOMINATE will not depend on the distribution of roll calls.

8. Nonetheless, Poole and Rosenthal (1987) show that the coalitional process is more complex than two-party or three-party benchmarks (Weisberg 1978). Unidimensionality is more than a story of two parties voting against one another.

9. In any case, they do not have a *theory* of roll call votes but rather an empirical regularity, which cannot be compared with a theory.

10. Poole and Rosenthal (p. 955) define the 50% criterion as "the limit of the one-dimensional classification percentage as the number of true dimensions grows indefinitely." In fact, this result requires three additional assumptions. First, the dimensions must be equally important. Otherwise, the effective number of dimensions remains small, and the criterion does *not* converge to 50% (Koford 1989a, 957–58). An infinite number of dimensions, all equally important, seems implausible. Second, the dimensions must be orthogonal; correlated dimensions are equivalent to a smaller number of dimen-

sions. A large number of dimensions with purely random levels of correlation (i.e., at the .5 level of significance) could reduce to a smaller number of orthogonal dimensions. Simulations (as in Poole and Rosenthal 1991) with random draws of proposals from an *n*-dimensional unit circle can give these correlations and calculate lower-dimensional fits. Third, all votes must have exact minimum winning coalitions; for if the probability that a legislator will vote on a bill is a random variable less than one, the average winning vote will be greater than the minimum winning coalition. Even if legislators propose bills with expected minimum winning coalitions and perfect knowledge of legislator preferences, the fact that some legislators may not vote implies that the actual majority will be a random variable greater than 50%.

11. Poole and Rosenthal (1991, Table 2) use the empirical average majority of 65.9% as their theoretical majority and calculate correct classifications similar to those in my original article, using simulations for three-plus dimensions. Snyder (N.d.) examines roll calls caused by random shocks that cause divergence from one of the vertices of my *n*-dimensional cubes.

12. Poole and Rosenthal's discussion of the additional dimensions they find implies that these dimensions are not very important and that their statistical method may not be capturing any real factors (p. 5).

13. The proportion is $p = .1597/(.659 \times .341 \times 2)$.

14. If all legislators have the same relative intensity of preferences across issue dimensions, a linear transformation of the issue dimensions will assure circular indifference curves.

15. These alternatives could be at any distance from the center, so long as both P and P′ are equally distant from the center.

16. High *demands* for change by legislators with bliss points far from the status quo are something different (Koford 1989b, Figure 1), since legislators with bliss points far from the status quo have high demand for change even when all legislators have the same gradients.

17. Methods of estimating models with heterogenous agents are described in economics as the "aggregation problem" (Jorgenson 1990).

18. Studies that did not include ideology or a related variable are omitted. Numerous studies examined constituency and party but not ideology. Ideology is never mentioned in Collie's (1985) review of roll call voting studies.

19. Kalt and Zupan (1990) examine the nature of this residual; part of it acts like a legislator's personal value, and it is this part that is successful in estimating roll call votes. They find that the ADA scale worked well as "a choice along the liberal-conservative ideological spectrum." But they also found that the League of Conservation Voters and other "social" ideology variables did better than the ADA scale in explaining energy votes. They did not examine the *number* of dimensions; rather, they examined the specific nature of the social preferences involved in "shirking."

20. Each coalition faces a trade-off between choosing a position at the coalition median and a position at the coalition's central point.

21. Using predicted ADA liberalism ratings from Ladha 1984 on the Zupan residuals gives a similar pattern. Seven of the "ideology residual" values in Kalt and Zupan (1990, Table 2) are incorrectly labeled, according to Zupan (personal communication 16 Nov. 1990).

22. For example, "selection bias" by committees choosing which issues to bring for a vote could cause this unidimensional effect (see Snyder N.d.).

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Forthcoming in December

The following articles, research notes, and controversies have been tentatively scheduled for publication in the December 1991 issue:

John Bruce, John Clark, and John Kessel. "Advocacy Politics in Presidential Parties."

Josep M. Colomer. "Transitions by Agreement: Modeling the Spanish Way."

Charles Franklin. "Eschewing Obfuscation? Campaigns and the Perception of U.S. Senate Incumbents."

James Kuklinski, Robert Luskin, John Bolland, Milton Lodge, Kathleen McGraw, Pamela Conover, Stanley Feldman, and Arthur Miller. "Where Is the Schema? Schemas and Schema Theory in Political Science." A symposium.

Thomas Lindsay. "James Madison on Religion and Politics: Rhetoric and Reality."

Stuart Elaine Macdonald, Ola Listhaug, and George Rabinowitz. "Issue Strategy in Multiparty Systems."

Kathleen McGraw. "Managing Blame: An Experimental Investigation into the Effectiveness of Political Accounts."

Iain McLean, John Orbell, and Robyn Dawes. "What Should Rational Cognitive Misers Do?" A controversy.

Juan Molinar. "Counting the Number of Parties: An Alternative Index." A research note.

Edward N. Muller, Henry A. Dietz, and Steven E. Finkel. "Discontent and the Expected Utility of Rebellion: The Case of Peru."

Jonathan Nagler. "The Effect of Registration Laws and Education on U.S. Voter Turnout." A research note.

Richard G. Niemi, Stephen C. Craig, and Franco Mattei. "Measuring Internal Political Efficacy in the 1988 National Election Study." A research note.

Robert Powell. "The Problem of Absolute and Relative Gains in International Relations Theory."

Donald Searing. "Roles, Rules, and Rationality in the New Institutionalism."

Dennis M. Simon, Charles W. Ostrom, and Robin F. Marra. "The President, Referendum Voting, and Subnational Elections in the United States."

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THE SUPREME COURT AND DEMOCRATIC ACCOUNTABILITY

Judicial Review and the Law of the Constitution. By Sylvia Snowiss. New Haven: Yale University Press, 1990. 228p. $25.00.

Considering the tremendous volume and quality of the literature on judicial review, it is surprising when a scholar makes a new and significant contribution to analysis in this area. But that is just what Sylvia Snowiss has done in her recent work, *Judicial Review and the Law of the Constitution.* A variety of rich and important insights flow from this intellectual history of judicial review. Most significant is the clarity that is brought to the different variants of judicial review by showing their historical and intellectual roots.

Snowiss is very clear and persuasive in her argument that the understandings of the Constitution and the role of the Court have changed quite significantly over time. Snowiss identifies and draws fairly dramatic distinctions between three formative periods: from Independence to Federalist Paper 78, from then to *Marbury* v. *Madison,* and the period of Marshall's tenure on the Court. According to Snowiss, preconvention arguments regarding judicial review were heavily influenced by Blackstone's position that the legislature was supreme and the Court had little or no role with regard to interpreting fundamental law. After the 1787 convention the Court was perceived as a coequal branch and it became possible for the Court to "take notice of the Constitution" and to refuse to execute laws that were in clear violation of fundamental law (i.e., "doubtless case rule"). In this period, however, the Court could only act by refusing to enforce these "concededly unconstitutional acts." Ultimate reprisals were presumably left to the people, through either ordinary political processes or revolutionary acts. Subsequent to *Marbury* v. *Madison,* the Constitution underwent (as a result of the conscious efforts of John Marshall) a shift from fundamental law to "supreme written law"; and the Court eventually gained supremacy over "ascertaining and pronouncing what the law of the Constitution was" (p. 181).

This intellectual and political history of judicial review receives the bulk of Snowiss's attention. Her purpose is to show that John Marshall's influence on the evolution of modern judicial review cannot be overestimated, but that the significance of *Marbury* v. *Madison* has been. Indeed, Snowiss offers dramatic evidence to show that the wording of *Marbury* was not a departure from then-prevalent views (e.g., that the Court was not authorized to interpret the Constitution). Snowiss's point is that when read with late-twentieth-century meaning, *Marbury* appears to be saying far more than Marshall actually said at the time. Thus, Snowiss claims, *Marbury* is not an example of modern judicial review. More important to the development of modern judicial review is the way Marshall organized the judicial decision-making process (e.g., replacing seriatim opinions with a single, majority opinion), as well as the influence Marshall had in shaping the law through supremacy and commerce clause rulings.

In her final two chapters Snowiss departs from this otherwise historical approach. She argues that critics of judicial review (especially Thayer and Frankfurter) were unable to launch a successful critique of judicial review because of a failure to understand what Marshall had done (i.e., turned the Constitution into supreme ordinary law). This failure fatally cripples contemporary critiques of judicial review. Only Alexander Bickel has had limited success, primarily because he understood the role of the Constitution and the Court in articulating and defending fundamental values. Snowiss praises Bickel and the fundamental values approach for understanding the Constitution as fundamental values and principles, rather than as law (p. 211).

Far from attempting to mend the crippled critiques of judicial review, Snowiss claims that efforts to eliminate judicial review's policy component cannot succeed. Rather, she suggests that we find a way to justify judicial review; and she indicates that "the most promising justification is in terms of the ends pursued by modern constitutional law" (pp. 204–5). The ends of constitutional law are to establish legal and democratic accountability. Thus, Snowiss concludes, in order to be a viable source of constitutional law, the judiciary must engage in routine and ongoing policy making (pp. 211–13).

Snowiss provides an interesting addition to our understanding of the origins of judicial review. This additional insight certainly does not end the debate over judicial authority, but it does clarify the dimensions and nature of the debate by helping us better understand the roots of the disagreement.

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The Tempting of America: The Political Seduction of the Law. By Robert H. Bork. New York: Free Press, 1989. 432p. $22.50.
Battle for Justice: How the Bork Nomination Shook America. By Ethan Bronner. New York: W. W. Norton, 1989. 398p. $22.50.
Ninth Justice: The Fight for Bork. By Patrick B. McGuigan and Dawn M. Weyrich. Washington: Free Congress Research and Education Foundation, 1990. 326p. $19.95.
The Judges War. Edited by Patrick B. McGuigan and Jeffrey P. O'Connell. Washington: Free Congress Research and Education Foundation, 1987. 307p. $12.95.
The People Rising: The Campaign against the Bork Nomination. By Michael Pertschuk and Wendy Schaetzel. New York: Thunder's Mouth, 1989. 317p. $13.95.
Packing the Courts: The Conservative Campaign to Rewrite the Constitution. By Herman Schwartz. New York: Charles Scribner's Sons, 1988. 242p. $19.95.

Four years after the defeat of Judge Bork for a seat on the Supreme Court, winners and losers alike still rehearse the arguments and events and reflect on the lessons so hard learned in the summer and autumn of 1987. For a major portion of the community of activists in social issues, on both the Left and Right, the fight over Bork stands as a defining moment in an ongoing tug-of-war over the political and ideological balance of the federal courts and national institutions on an array of matters, from civil rights to abortion and the environment. It was a mobilization of grassroots and direct pressure on the Senate unparallelled in the history of interest groups in the United States and certainly on judicial nominations. Never before or since had so many organizations of such diverse stripes and structures come together to do battle over a judicial nomination or any other issue.

Yet the news here is not the mobilization of such forces but, rather, their quantity. Severe and overt competition among organized interests in the Senate over seats on the Court and the federal bench has surfaced from time to time in U.S. politics at least as far back as the early 1900s. Scholarship in history and political science reflects some of these fights; the archival record shows many more. Organized interests have mobilized in a visible manner on dozens of federal judicial nominations since the turn of the century and behind the scenes in many, many more instances. In sum, the clash of organized interests in the politics of federal judgeships is nothing new.

It is no simple matter to trace the sources of these controversies with any degree of precision. But normally, political conflict over federal judicial nominations has arisen under several fairly well understood circumstances. In the final phase of a term, as the opposition begins to smell victory in the near future, a president often faces a difficult task in gaining confirmation for a nominee to the Supreme Court or lower courts. If the chief executive's party does not control the Senate, with all of the attendant advantages that implies, he has often had rough sledding. And of course, the character, partisan associations, political stands, and qualifications of the nominee himor herself can generate controversy and organization opposition. Political conflict over judicial nominations rises and declines as these and other conditions appear, come together, and then recede from the political landscape. It is not a constant feature of U.S. politics. The constellation of political conditions at any particular time, of course, serves as a host for the growth and activation of various organized interests.

Yet organized conflict did reach a new level in the 1980s. It had become institutionalized over the two decades since the defeat of Abe Fortas in 1968. Until the late 1960s and early 1970s, apart the NAACP and the AFL-CIO, few organizations had taken a continuous interest in nominations to the federal courts or even the Supreme Court; and relatively few organizations until then had used litigation as a central part of their political strategies and tactics. This period marked the establishment of a tremendous number of new organizations focused on civil rights, environmental policy, mental health, consumerism, disabilities, and so on. Few scholars took notice of these developments; but liberal activists and organizations gradually built up an infrastructure of personal and professional contacts, political and scholarly expertise, financial resources, and credibility in the eyes of senators on the issue of federal judicial nominations. Liberal organizations had by the middle of the 1980s moved away from hit-and-miss consideration of, and occasional opposition to, judicial nominees to a more systematic and institutionalized process of screening and decision making. Thus, the campaign against Judge Bork did not just happen. It represented the culmination of years of organizational investments in human and financial resources.

The Bork campaign commands our attention for a number of reasons. First, to an unprecedented extent, it focused large segments of the citizenry on the crucial constitutional conflicts of our time. Second, the campaign in a sense summarized in one clash the dozens of smaller battles between the Reagan administration and the individual constituencies within the opposition to Bork; it expressed the accumulated frustration of hundreds of organizations interested in diverse areas of public policy. Third, after years of defeats, the victory demonstrated the considerable strength of the coalition of organizations on the liberal side—in particular, the Leadership Conference on Civil Rights. Fourth, the campaign mustered against Judge Bork has become a model of how to run a successful lobbying effort. Interpretations of events as momentous as those in the battle over Bork tend to take on a life of their own in the memories of the people who made them happen. Liberal activists now speak of "doing a Bork," that is, replicating the strategies and tactics in other areas. Conservatives, too, have picked up on the campaign as a model; and should the political situation reverse, we would likely see the Right serve up a set of activities along the lines of the anti-Bork coalition.

President Reagan's attempts to reshape the federal judiciary and the campaign against Judge Bork have spawned a large, growing, and often impressive corpus of articles and books above and beyond the usual and repetitive fare in the law reviews. It provides impressive documentation of controversies over judicial politics in the 1980s—a real boon to scholars. With one exception among the books I consider here, committed activists on the Left and Right have written this record. This, of course, has costs and benefits. On the positive side of the ledger, no one could match in the intimate knowledge and access to the inner workings of the campaigns on the Right and the Left on Bork and previous nominations. On the negative side, of course, scholars will naturally view the interpretations of the events in a skeptical light. But even if we apply a heavy rate of discount to these books, we have a rich and unprecedented trove of information about direct and grass-roots lobbying and the politics of judicial nominations. And it helps to have books on both sides.

Two of the books I consider here—Herman Schwartz's *Packing the Courts* and Patrick McGuigan and Jeffrey O'Connell's edited *Judges War*—help to place the Bork campaign in context. Conservatives in the Reagan administration, according to Schwartz, set out to reverse the Supreme Court's decisions on a number of controversial issues, from abortion to school prayer. To accomplish this goal, conservatives have sought to "pack" the federal courts at all levels with ideological soul mates. Schwartz discusses a wide range of controversial nominations and the growth of organized opposition among liberal interest groups. McGuigan and O'Connell's *Judges War* represents the conservative version of the same set of events. Conservatives see the genesis of the "war" over the courts in the "ideological" nominations of the Carter presidency; the application of "litmus" tests began then and not in the 1980s. In opposing many of President Reagan's well-qualified nominees on philosophical grounds, according to conservatives, liberal interest groups have changed the traditional rules of the game. So on the one hand, liberals view the recruitment of judges in the Reagan administration as a major point of departure; and on the other hand, conservatives see it as part-and-parcel of the office's historical prerogatives. The stark contrast between these interpretations provides an excellent indication of why fierce, sometimes public, conflict has so often characterized the politics of federal judicial nominations in the last decade.

Judge Bork's *Tempting of America* constitutes a vigorous defense of "originalism," his model of constitutional interpretation; an indictment of the "result-orientation" now regnant in U.S. law; and a personal account of his experiences in the Senate's rejection of his nomination to the Supreme Court. But it is much more than Judge Bork's judicial testament. For conservatives who wish to reform the judiciary it is an important document, for Judge Bork means to take his case to the reading public—just as Schwartz, Laurence Tribe, and others have written popular tracts. For political scientists who wish to understand the politics of the federal judiciary, it is another milepost in the continuing "war of the judges."

Pertschuk and Schaetzel's *People Rising* takes us from the beginning through the aftermath of the rejection of Judge Bork and tells us in great detail about every facet of the campaign. Pertschuk is a veteran of many campaigns, especially on consumerism, and now directs the Advocacy Institute, an organization designed to train lobbyists for liberal causes. He and Schaetzel enjoyed marvelous access to the activists and to the documents and information on which the campaign relied in the coalition against Bork.

Perhaps more than anything, *The People Rising* illustrates the complexity of organizing and maintaining an effective coalition on an important issue. Coalitions on the Left and the Right are not monoliths. We see organizations in the coalition against Bork engaged in debate and wrangling over strategies and tactics, in contrast to the united front displayed in public. Leaders of black organizations saw Bork's nomination as a question of civil rights and at the outset did not want to bring in abortion rights groups. Traditional tensions within the Leadership Conference on Civil Rights arose again. For reasons of organizational maintenance, some members sought to carve out an individual position.

Opponents of Bork amassed an incredible amount of research, and Pertschuk and Schaetzel accord much importance to these efforts. The depth and timeliness of the research made the coalition more credible and helped to define the terms of the debate; even more important, the wide diffusion of the research to activists and rank-and-file out in the pivotal states made for much more effective mail from the grass-roots. Letters from the anti-Bork coalition, in contrast to the usual boiler-plated fare, showed evidence of some understanding of the issues at stake. Each part of the coalition conducted research independently. That meant much redundancy; but it also meant additional credibility, as diverse constituencies weighed in against Judge Bork. On the coalition at the grass roots, Pertschuk and Schaetzel emphasize the unusual combination of local initiative and professional direction from Washington. For the first time, the Leadership Conference and the Alliance for Justice hired professional organizers to coordinate the campaign at the grass roots. Yet many local activists began to organize coalitions and go to work without prompting from the national coalition. Local coalitions often pushed ahead of the national coalition.

The anti-Bork coalition used the most modern methods of communication. For example, in framing the message in the campaign against Bork, the coalition used focus groups, polling, and advertising agencies. It was able to test a series of themes and choose the most appealing. These methods, interestingly, caused consternation not only among conservatives, who cried foul, but also among many ideological purists in the coalition. From beginning to end, the coalition inundated the mass media with information. Early in the campaign, it sent out many memoranda based on the legal research on Bork's record. Later, the materials for the press emphasized the themes produced from focus groups and polling.

Did the campaign change the nature of confirmations for the Supreme Court? For the near future, Pertschuk and Schaetzel argue, nominees will face careful scrutiny from the Senate. This will depend, in my view, on the underlying conditions of U.S. politics, some of which I have mentioned. Divided government, for example, provides a fertile environment for the development of organized opposition to nominations. That, in turn, forces the Senate to exercise care in screening.

Did Judge Bork receive fair treatment? Pertschuk and Schaetzel acknowledge some of the excesses of the campaign, especially at the grass roots, but they defend the essential fairness of the arguments. The answer to this question depends, in large part, on whether one concedes the legitimacy of ideological scrutiny and of public involvement in the selection of judges. For those who think it inappropriate for senators to take ideological considerations into account or who frown on broad public discussion, the campaign against Judge Bork constituted a travesty. For others it was evidence of the vibrancy of constitutional democracy.

Victory in the fight over Bork had important consequences beyond the Supreme Court; it has changed the nature of future campaigns for liberal causes. It promoted unity, integrated national leadership and outside networks, used the full range of professional skills and techniques, and renewed hope among liberal activists. Most important of all, I think, it created a model of how to run a sophisticated and disciplined campaign.

Did the campaign against Bork make a difference? Those who worked against Bork express ambivalent feelings on this point. Leaders of organizations and activists naturally wish to believe that their considerable efforts make some difference; that is why they worked so hard in the first place. To assign too much credit to interest groups, however, fits in with the pro-Bork coalition's claims about the inappropriate influence of "special" interests—the notion of interest groups as a lynch mob. Pertschuk and Schaetzel conclude, "The principal contribution of the outside campaign was to force the Senate to take seriously its constitutional responsibility to examine the Bork nomination." Then, too, the intensity of the broad-based outside campaign signaled to senators the significant political costs of a vote for Bork. Senator John Breaux of Louisiana summarized the response of many of his colleagues: "If you vote against Bork, those in favor of him will be mad at you for a week. But if you vote for him, those who don't like him will be mad at you for the rest of their lives." I think Pertschuk and Schaetzel understate the influence of the coalition. In the past and in the wake of the Bork nomination, senators have shown little taste for conflict over nominations. Without the massive amount of research, mobilization, and lobbying, the Senate would not have taken on this time-consuming and politically treacherous issue.

In *Battle for Justice* Ethan Bronner, a journalist for the *Boston Globe*, tells the story of the nomination from the angles of all important participants—Judge Bork, senators on both sides, interest groups, and the White House. He has interviewed virtually all of the central players and has inspected a rich set of documents. Despite a few errors and a superficial portrayal of many constitutional issues, *Battle for Justice* will serve political scientists well as the most comprehensive of this set of books. Here we learn much about the personal dimension of this battle, of its cost to Judge Bork and his family.

Bronner exposes the great tensions between the pragmatists in the White House and the conservatives in the interest groups and the Department of Justice. If Bronner's account is correct, the White House did very little to push Judge Bork's nomination until half way through September, long after the coalition had set its plan into motion. Despite a reputation as an effective lobbyist, President Reagan spoke with a small number of senators and few of the most crucial targets, the Southern Democrats. Over the objections of conservatives, strategists in the White House decided to sell Bork as a moderate in the style of Lewis Powell. Judge Bork's constitutional views provided an intellectual framework and justification for much of the conservative agenda of the 1980s, which, of course, is why President Reagan nominated him and why conservatives on the hard Right saw him as an essential part of the revolution on the Court. This did not work. Judge Bork's long list of emphatic writings made it impossible to deny his conservatism; and when he seemed to back away from previous statements, he came off as a trimmer in the eyes of some. Bronner's fascinating and detailed narrative of the conflict behind the scenes over Judge Bork's preparation for, and performance in, the hearings helps to explain why he did not take some of the more obvious steps to allay the fears of senators on the fence post.

The White House made a few personal contacts, slowly answered the charges against

Bork, and waited for the hearings. It did not put significant pressure on senators in Washington, and it made only feeble attempts to generate pressure from the outside. The performance of the White House in lobbying on the Bork nomination stands in stark contrast to its feats on many other issues in the 1980s. Howard Baker and others in the White House either did not understand the nature of the battle in which they found themselves or chose not to devote the resources required to gain a victory against formidable opposition. Why did Bork lose this fight? Did the interest groups do it? Bronner tries to tell the story without coming to grips with this question. The weight of Bronner's narrative seems to implicate Bork himself for a rigid response in the hearings and lack of restraint in previous writings and the White House for failure to understand or act on the dire situation.

McGuigan and Weyrich in *Ninth Justice* do not shrink from placing blame or drawings lessons from this battle. For conservatives of the New Right, Judge Bork was and is a very special person, the major intellectual force in the movement. Patrick McGuigan, former director of the Judicial Reform Project of the Free Congress Foundation, and Dawn Weyrich, a reporter for the *Washington Times*, have written an important book from the perspective of conservative interest groups, very much in the same vein as *The People Rising*. *Ninth Justice* provides an extraordinary look inside the New Right; for McGuigan, with Daniel Casey of the American Conservative Union, coordinated the efforts of conservative interest groups for Bork. The book is deadly candid about issues and personalities and has no doubt ruffled many feathers among conservatives. McGuigan and Weyrich provide a nearly blow-by-blow account of the battle, especially the mobilization of conservative interest groups, interspersed with McGuigan's personal reactions and activities. It is the most detailed account of how lobbyists make decisions about how to allocate effort among senators; McGuigan and Weyrich reprint several of the many head counts and lists of targets the pro-Bork coalition took over the span of the campaign and recount a number of the meetings with senators.

McGuigan and Weyrich locate Judge Bork's difficulties in the White House and in the huge coalition arrayed against him. Members of the staff responsible for the confirmation attempted to sell Judge Bork as a moderate conservative and then invested little in the sort of high-pressure lobbying McGuigan and others thought necessary. To make matters worse, President Reagan, who had succeeded so magnificently in other legislative battles, sat out the campaign for Bork until too late in the game. Baker and others in the White House, as a result of the flawed strategy, discouraged the New Right from going public early in the campaign. Efforts to mobilize the constituencies of the New Right would, in this view, only give credibility to the claims of the anti-Bork coalition. Organizations in the New Right of course did mobilize their supporters, but the resistance of the White House slowed them down.

The campaign against Bork, which Pertschuk and Schaetzel celebrate as a classic, McGuigan and Weyrich denounce as the sophisticated use of modern technologies in the distortion of a distinguished man's public and private views. Focus groups and polls used to create a simple set of messages and advertisements come in for particular criticism. Conservatives take an especially dim view of People for the American Way, precisely because it has often emphasized the use of polling and advertising in fights over judgeships and civil rights and liberties. Those who fought against Judge Bork have changed the "rules of the game"; in the future, nominees who do not pass the litmus tests of liberal interest groups will face slick, well-financed campaigns of distortion. The old, inside game of judicial nominating politics has, according to McGuigan and Weyrich, given way to a highly partisan and ideological style.

Organizations on the Right, like those arrayed against Bork, had difficulties in building an effective coalition; and *Ninth Justice* tells of some of these. For example, conservatives in the 721 Group—an ongoing coalition of around 20 right-to-life, religious, and law enforcement organizations—could not persuade the National Rifle Association, an ally on many issues, to support Judge Bork. Ironically, Bork's restrictive views on search-and-seizure worried the leadership of the NRA. It could have contributed additional vitality at the grass roots and, perhaps more important, ties to a number of senators based on financial clout. Conservative interest groups are not monolithic. Equally serious, corporate lobbies did little or nothing to assist Judge Bork, despite the efforts of the White House to mobilize this important constituency. Some trade associations did lobby for Bork, but the efforts were half-hearted and not credible in the Senate. Corporate lobbies see little at stake for them in the social issues so salient to the New Right, so business rides freely in this and other such situations; it could rest contentedly, for whomever President Reagan ultimately placed in Justice Powell's chair would be a friend of U.S. business. Failure to mobilize big business left Judge Bork with the active support of 20-30 organizations and the indirect support of perhaps another 70 or so on the New Right—against a mammoth coalition of over 300 organizations, 50 or 60 of which invested a substantial amount of resources. This lack of ideological commitment angers many in the New Right, and it is a continuing problem.

The defeat of Judge Bork seared the psyches of many committed conservatives; and McGuigan's recollections in *Ninth Justice* leave no doubt of the personal impact of this campaign. In the afterword McGuigan sets forth a list of lessons from the Bork campaign. These lessons, if followed, presage a high level of controversy over nominations in the 1990s. For one, McGuigan warns conservatives to prepare to wage "the legislative equivalent of a senatorial campaign, complete with high dollar media expenditures, vigorous negative advertising and polemic theatrics." For another, he recommends "broad ongoing coalitions in the states comparable to those formed on the Left. . . . Such coalitions must include . . . businessmen, Christian activists, law enforcement and others." Perhaps conservatives have internalized these lessons. In the wake of the defeat of Bork, conservatives prepared a detailed plan of action after the fashion of the anti-Bork campaign; but Kennedy's smooth confirmation did not require its execution.

Will this conflict over federal judicial nominations persist? Those on the Right and the Left involved in monitoring nominations think so, and that makes for something of a self-fulfilling prophecy. The intensity of organized conflict over federal judgeships will, as I said earlier, largely depend upon the continued existence of divided government and judicial activism, both of which initially gave rise to the controversy. The rise of watchdog groups means the institutionalization of organized conflicts, so we are likely to see a continuation of the battle for justice.

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POLITICAL THEORY

The Logic of Political Belief: A Philosophical Analysis of Ideology. By Ian Adams. Savage, MD: Barnes & Noble, 1989. 145p. $39.75.

To take up Ian Adams's *Logic of Political Belief* is to enter a time warp and to reemerge on the far side of 1953 with T. D. Weldon's *Vocabulary of Politics* in hand. It is to return to a kinder and gentler world in which Michel Foucault has yet to ask whether all claims to science are affirmations of power; in which Jacques Derrida has yet to wonder whether language is a self-referential complex of free-floating signifiers; in which Richard Rorty has yet to inquire whether the positivist appeal to hard facts is merely the Western philosophical tradition's latest gimmick designed to anchor knowledge in unquestionable foundations.

Contemporary philosophers, Adams argues, have left our thinking about ideology in a state of chronic confusion. Insufficiently rigorous in adhering to Hume's fact-value distinction, too many have smuggled "normative" commitments into their avowedly objective analyses. "It is this failure of ethical detachment that is the root of the failure to analyse ideology" (p. xii). Citing Hobbes in defense of his contention that "values" are emotional ejaculations without claim to cognitive standing, Adams sets out to offer a truly "philosophical" account of this "inherently defective mode of thought" (p. 140).

To clear the ground necessary for this operation, Adams makes two moves. First, he rejects as partisan all Marxist, liberal, and conservative accounts of ideology and condemns as incoherent all sociological accounts that proclaim the unbiased validity of their analyses while in the same breath characterizing all thought as mere epiphenomenal reflection of underlying causes. Second, Adams argues that an objective understanding of ideology can only be generated by a real "academic discipline," that is, by a scholarly community whose members universally endorse a self-enclosed system of fixed rules that, when applied to any issue, will definitively adjudicate between true and false claims to knowledge. In short, although failing to furnish any detailed statement of the decision procedures that define the logic of philosophy, Adams simply recapitulates the weary positivist repudiation of all "metaphysical" enterprises (such as political theory, as traditionally practiced) on the grounds that formal logic, mathematics, and perhaps economics are the only sources of genuine knowledge.

His rivals slain, Adams turns to the logic of ideological belief. What distinguishes this form of understanding is the way it relates fact to value: "Description and evaluation are fused in the same concepts, with the effect of turning concepts which retain their descriptive appearance into evaluation ones with no genuine descriptive force" (p. 42). The ideologist is necessarily a charlatan; for the success of the ideological enterprise requires that it present itself as what it is not, that is, as an exclusive and certain representation of the world's truth. But because there can be no decision procedures that will arbitrate between the pseudo-descriptive concepts comprising such self-validating ideologies, none can legitimately claim superiority over another. "Ideological theory cannot have a real grip on the world" (p. 103); and so the adherents of different ideologies, as prisoners of their respective tunnel visions, necessarily prove "unable to enter a dialogue with each other" (p. 137).

It is tempting to debunk the argument of this book by exposing its own ideological presuppositions. One might, for example, ask whether Adams's praise for Hobbes's allegedly nonideological method intimates the politics implicit in a work that hopes to temper the anarchy of impassioned relativists through rigorous scientific discipline. But such an analysis would divert from the main issue. The overall plausibility of Adams's argument turns on whether one still finds intellectually persuasive the unregenerate positivist worldview reflected in claims like the following: "To describe is to state what is there, what is in the world independent of our describing it; it is to state facts. . . . Since there is some sort [what sort?] of correspondence between descriptive terms and reality, disputes over the accuracy of a description can be settled by checking it against the facts" (p. 36). If you believe that the world can be precisely and exhaustively divided into subjective values and objective facts and that we have available to us a language whose purity assures its neutral grasp of the latter, you may find Adams's argument sound. But if you regard belief in the existence of such a language as an article of faith that stands or falls with a belief in God, you will not. In the last analysis, perhaps the truth of Adams's critique of ideological thinking is confirmed by my inability to find credible the world in which he lives.

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A Discipline Divided: Schools and Sects in Political Science. By Gabriel A. Almond. Newbury Park, CA: Sage, 1990. 348p. $39.95 cloth, $18.95 paper.

Gabriel Almond has been a witness to most of the developments in modern political science. More than that, he has been a vital part of those developments, as both an organizational and intellectual leader. We are fortunate that a scholar of his depth, range, and vision not only remains interested in assessing contemporary developments in political science but has the intellectual scope and background to evaluate these currents against the longer history of the discipline.

This book brings together a number of Almond's papers, written during the late 1970s and throughout the 1980s, most previously published, all dealing with the general theme of conflicting explanatory schemes and perspectives in the discipline. In addition, the book includes a biographical sketch Almond wrote about Harold Lasswell and excerpts from an oral history conversation between Almond and Richard Brody, focusing on Almond's days at Chicago and his reflections on "the Chicago school."

These papers provide an important perspective on the intellectual history of political science since World War II. Almond identifies with the political behavior movement, the primary tenet of which (the development of empirically based theories of political action and processes), he argues, has always constituted "mainstream" political science. He sees the political science of the 1950s and 1960s as basically a unified discipline with a coherent body of knowledge, with new work being cumulative and building upon the base of prior scholarship even where the methods and foci of interest changed in dramatic ways. Certainly not all political scientists described themselves as "behavioralists," but the shared commitment to the scientific approach exemplified by behavioralism created a unity of purpose and a sense of cohesion in the discipline: "An appropriate metaphor for the state of political science at that time perhaps would be the 'young Turk–old Turk' model. . . . But we were all Turks" (p. 14).

Almond reviews most of the intellectual threads of the discipline that have challenged the dominance of the political behavior movement. As the title suggests, his interpretations of most of these intellectual challenges tend to be negative: rather than making cumulative contributions to knowledge of political processes, he sees them largely as discontinuous, noncumulative, and divisive. Some are criticized for being political in an ideological sense, others for having too partial and narrow a view of important variables or for being almost entirely uninformed by the prior scholarship of the discipline. The burden of several of the papers is that political processes are not sufficiently predictable to support rigorous deductive theory to the extent desired by advocates of the public choice school and other attempts to move beyond the empirically based theory of behavioralism to mathematical and deductive approaches to the description of political action.

Almond gives concerted attention to some of the major works challenging pluralism and political culture theory, those espousing "dependency theory," and the movement known generally as "the new institutionalism" or the "return to the state." About the last two in particular, he argues that each offers only a very partial view of the political processes they attempt to analyze; and to the extent that they focus on important variables (e.g., governmental structures), these variables were either explicitly or implicitly included in earlier work. Challenges to pluralism and political culture theory are found wanting empirically. A frequent complaint is that many proponents of challenges to "mainstream" work have not adequately reviewed prior literature; had they done so, they would have realized that their critiques were unfounded. He praises some of this literature; but by and large, he feels that many scholars who represent these challenging points of view have rejected a heritage with which they are woefully unfamiliar.

Almond became a political scientist in an era when the vision of a science of politics was much more unified than it is today. While he recognizes in an abstract way that challenges to the prevailing paradigms can be a sign of intellectual vitality, he tends to view the particular developments treated in this book as contributing primarily to a fractionation of the discipline. While at times his detailed analyses and rebuttals of challenging perspectives sound defensive, the vision from which he argues deserves thoughtful consideration. He seeks explanations and theories that are more complex than those offered by some of the schools he criticizes. Not only should more variables be included to reflect the complexity of political behavior, but room needs to be made also for unique occurrences and unusual coincidences, such as the critical roles played by particular leaders or unique circumstances in explaining particular events or processes. Sometimes such considerations will preclude rigorous predictive or deductive sorts of analyses. Most important, we need to take seriously the work of previous generations of scholars before we hasten to proclaim a "new" paradigm or approach; we need to know the work of those who have gone before in order to build upon it. Almond's analysis is based on a vision of a discipline unified over time and across subject areas that is frequently missing in the work of the later generations. It is a vision that gives important meaning to political science as a discipline.

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Political Equality: An Essay in Democratic Theory. By Charles R. Beitz. Princeton: Princeton University Press, 1989. 253p. $25.00.

A more pointed title of this book would be *Democracy As Fairness*, since Beitz does for democracy what Rawls did for justice—not in the sense that Beitz's work is equal in stature to Rawls's (it is deliberately less grand in substance and more limited in scale because focused on issues of concern to political scientists) but rather in the sense that fairness occupies a central place in both the analysis and justification of political institutions. As Beitz puts it, consistent with his method of "reforming definitions": "Were it not for a desire to respect the traditional usage, we should dispense with the phrase *political equality* altogether and refer instead to the more fundamental value of political fairness" (p. 218). True enough that democratic institutions treat citizens as equals, but this does not imply that democratic equality is fair. An argument is needed to make the case that democratic procedures satisfy the requirements of political fairness and thus treat citizens equally *in the appropriate way*. Beitz's project is to supply just such an argument, the crux of which is the social contractarian idea that fair terms of participation are those that pass the principled and reasoned scrutiny of each and every citizen, assuming a shared desire to agree collectively on procedures designed to settle conflicts and advance common interests.

Beitz's strategy is the negative one of surveying alternative defenses of democratic equality, pointing up their shortcomings and building from the wreckage his preferred theory, which he believes is more coherent and best accords with the intuitions of citizens in existing liberal democracies. These alternative theories are not straw men. Beitz confronts their best representatives; provides crisp, reliable readings of them; and for the sake of argument, contributes to their force by shoring up their defenses against standard objections. Not surprisingly, given Beitz's deontological slant, at the top of this list of rejects are consequentialist theories, which he divides into social welfare and social choice varieties. Though they define welfare somewhat differently, they are alike in taking on the task of assessing decision-making procedures in light of how well they "equally" aggregate welfare. Happily, Beitz's coverage of a very technical literature is accessible to political theorists lacking training or interest in the calculus of formal modeling; his analysis is sophisticated without being intimidating. His criticism of welfare theories is Rawlsian: aggregative theories do not take individuals seriously, and social choice theories do not provide normative criteria for discriminating among claimants for inclusion in the aggregation. Especially bothersome about social choice theories is their treatment of preferences as exogenously given and then exhausting theoretical energy in the search for an effective social welfare function. For Beitz, this approach ignores the ethically significant role of democratic decision-making procedures in creating and altering preferences. These preferences should be seen as endogenous products of a political system that encourages public deliberation of issues in both agenda setting and voting.

Beitz also criticizes procedural theories that identify equal treatment with giving individuals equal chances to influence outcomes, no matter what they may be. Pure procedural theories have the advantage of securing a pivot for the value of impartiality, understood as public recognition of the equal worth of persons. But they are problematical because they underestimate in an important way the stakes that persons have in policy outcomes. Indeed, impartiality might be violated if certain consequences ignore some persons' urgent needs. It is here that Beitz qualifies the deontological perspective that informs his political philosophy, revising even more than Rawls their common Kantian heritage.

Beitz dubs his democratic theory *complex proceduralism*. He attributes to all citizens interests in recognition, equitable treatment, and deliberative responsibility. These interests regulate the choice and operation of democratic procedures, and their violation undermines the normative support that stems from unanimous agreement. Agreement by whom? By each and every individual counterfactually committed to procedural fairness (not seeking procedural advantage) and actually possessed of particular knowledge of individual circumstances and an interest in avoiding "unnecessary" harm. Recognition conveys the idea that the polity acknowledges the equal individual worth essential to self-respect; deliberative responsibility, which requires formal openness and the backup institutions that render it practical, is a necessary condition for confidence in the integrity of the polity; equitable treatment, which safeguards urgent or vital interests, is important for stability and prevents "serious and recurring injustices when there is an alternative available that would be less likely to do so without introducing countervailing harm of other kinds" (p. 113). This view, heavily dependent on T. Scanlon's "Preference and Urgency," is that justice and interest tend to converge in the well-ordered polity. The argument is not drawn conclusively; nor perhaps can it be, since it deals with essentially contested concepts.

In the last part of the book, Beitz applies his theory to concrete issues like representation, the formation of political agendas, and the funding of elections. He argues that regulative interests can be satisfied by single-member territorial districts; the popularity of proportional representation among many advocates of political equality results from a narrow emphasis on aggregation. This, as well as Beitz's analysis of fair representation and legislative districting (racial and party gerrymandering), is one of the surprising turns taken by what is plainly an egalitarian theory. Throughout his consideration of these issues, Beitz is careful to stress the pluralistic nature of the three components of the theory, calling for balancing in lieu of social and historial contingencies. The theory is not deductive.

At the end of what is one of the finest contemporary defenses of democratic practice available, Beitz muses about his method and worries that his failure to provide foundations for regulative interests might give readers occasion to object that his case is relativistic because it depends on an intuitive appeal to people already living in liberal democracies. I'd worry, too; and I leave it to readers to evaluate Beitz's way of relieving anxiety about ungrounded theories (pp. 221–25). What concerns me is that some of our best political philosophers, who deny neither the possibility of supplying foundations nor the importance of doing so, opt to pursue the "method of avoidance," withholding their talent from doing the necessary and, ultimately, decisive work.

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Secular Utilitarianism: Social Science and the Critique of Religion in the Thought of Jeremy Bentham. By James E. Crimmins. New York: Oxford University Press, 1990. 361p. $68.00.

Only the most alert reader of Halévy's *Growth of Philosophic Radicalism* (1928) would have guessed at the importance of the analysis of religion in Bentham's system. The same reader would need to have been rather sensitive to appreciate its importance in the description Leslie Stephens gave of it in his *English Utilitarians* (1900); yet Stephens was a free-thinker. These two studies are, despite their age, the most important and competent of the overall surveys of Bentham's thought. It can now be shown from Crimmins's study that they missed the main point of his system. Why?

Bentham made the mistake of entrusting his posthumous *Works* (11 vols., 1838–43) to his secretary John Bowring, a Unitarian minister, who deliberately suppressed in the edition the three major works by Bentham on religion: *Church-of-Englandism and Its Catechism Examined* (1818), *An Analysis of the Influence of Natural Religion on the Temporal Happiness of Mankind* (1822), and *Not Paul, but Jesus* (1823). Further, he did not edit any of the material on this subject from Bentham's voluminous manuscripts and suppressed all mention of the matter in his inept biography of Bentham to be found in the last two volumes. It should also be noted that Etienne Dumont, Bentham's superb French editor, also toned down the passages on religion in many of the more general treatises published by him between 1802 and 1823.

Later scholars seem to have followed in

Bowring's path; or else they simply did not read the material (which is exceptionally rare in bibliographic terms); or else, if they did read it, they read it as a response to the political disappointments of Bentham in the 1790s and 1800s, events that moved Bentham to a new radical position in politics. This type of reading of Bentham will now be impossible to do unless new evidence can be discovered to counter that produced by Crimmins.

What makes *Secular Utilitarianism* so important as an interpretation is the extent to which it is based not only on a close reading of what Bentham did publish but on a wide-ranging knowledge of the vast corpus of Bentham manuscript material on this and related subjects and on the development of theological utilitarianism earlier in the eighteenth century in the work of John Brown, William Paley, and others. The result of this investigation is the conclusion that Bentham's religious radicalism (or rather his atheism) was formed as early as 1773–74, that it is of a whole with his philosophy (a mixture of empiricism, materialism, and nominalism), and that it both underpins and explains his turn to political radicalism in 1809. The importance of Bentham's philosophy to the rest of his political thought had been noted in Ross Harrison's *Bentham* (1983); what Crimmins does is to show how important it is to an understanding of the radicality of Bentham's political philosophy. To put it succinctly, "In the rational society . . . religion is superfluous to ethics and need not be a consideration for the legislator" (p. 84).

Bentham had a project that included the total reconstruction of society on utilitarian grounds. To this end religion was not only not needed; it was positively harmful to human happiness and development. "Where religion was opposed to utility it was pernicious, where it agreed with it, it was entirely superfluous" (p. 98). This conclusion was not the result of the blind opposition of the clergy of the Church of England to nearly all the reform proposals of Bentham, though such antagonism undoubtedly sharpened the tone of his attacks on them (see chap. 6); it was the *logical* result of the whole nature of Bentham's thought.

This conclusion does not come from a tortured interpretation of a small handful of texts. The material on religion amounts to several thousand pages. Further, once the nature of Bentham's commitment to this end is realized, many of the early passages in the published works reveal new aspects. Nor is Crimmins's interpretation the result of his fondness for the position. His concluding chapter, "Atheism and the Secular Utilitarian Society," makes this clear enough in the nuanced critique he provides of the inadequacies of Bentham's attitudes.

Despite the occasional passage that reproduces Bentham's rather tortured prose style, the work as a whole is remarkably clear and well written. It is also a study that places Bentham's thought in the context of the thought of his period, and explains the characteristic shape of the version of utilitarianism that J.S. Mill developed. This interpretation will provide an important context for future readings of Bentham.

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Creative Marginality: Innovation at the Intersections of Social Sciences. By Mattei Dogan and Robert Pahre. Boulder: Westview, 1990. 278p. $34.85 paper.

The central idea of this volume, written by two political scientists, is that intellectual innovation is more apt to occur at the periphery of disciplines. With expansion and development in political science, for instance, fragmentation into subfield specialties begins to occur; and when specialists from one discipline eventually meet up with those from another who are working on similar problems, conditions are ripe for innovation. The result most often is a hybrid field consisting of recombined fragments. Since few unfilled gaps now exist between social sciences, the future is with hybrids rather than with disciplinary growth per se. This is not necessarily cause for alarm: traditional academic departments are biodegradable and fertilize succeeding intellectual developments. Hybrids themselves can wax and wane: for example, devotees of Marxist political economy and of statistical analysis (regression and factor analysis are explicitly mentioned) have failed to transmit their enthusiasm to a new generation. Hence, they remain in intellectual cul-de-sacs despite innovative beginnings.

By their own testimony, what Dogan and Pahre describe is normal rather than revolutionary science; and it progresses through the efforts of masses of workers rather than the "star system," which is roundly denounced as detrimental. All workers great and small contribute to the "patrimony," that common product of past scientific labor that provides the apperceptive mass of concepts and procedures with which all must be acquainted. This accounts for the many incidents of simultaneous discovery: innovation is at times "in the air," and virtually all active workers have a sixth sense as to where to look and which hypothesis to test next. It is in this way that "chance favors the prepared mind" (Pasteur) and that the logic of discovery (Peirce's abduction) plays its most important role.

Creativity occurs at the margins in large part because of the "paradox of density." The subfield of U.S. politics, to take one of the authors' illustrations, is glutted more because of hiring practices driven by student demand than by the lure of unsolved scientific problems. The consequence is sterility marked by excessive jargon, overemphasis on method, and theoretical hairsplitting—unproductive activities that are able to persist because the academic audience has sufficient density to support them. A move to the specialized and less-dense periphery and away from the madding crowd is a move away from shared assumptions and stifling collegiality and into a setting in which ideas have greater animation and innovation becomes more likely. By the time a newly developed hybrid field becomes popular and large numbers of scholars move into it, the major innovations have already been made.

Hybrids are not mere syntheses, which can take place without scientific advance; nor are they simply specializations, which are ultimately self-destructive; nor are they the result of "interdisciplinarity." The latter, according to the authors, involves two or more entire disciplines, whereas hybrids are recombinations and interpenetrations of fragments (e.g., biopolitics and political psychology). The final chapters are devoted to four such "crossroads": sociology and history, the social and life sciences, political economy, and economics and psychology. Prior to that, the authors discuss the diffusion of concepts and methods, the influence of new theories, and the cross-disciplinary repercussions of scientific findings. They also consider the impact of technology but mention only in passing the technical advances, such as electronic networks, that are making hybridization increasingly possible. Finally, they introduce three ideal-type social scientists—the pioneer, the builder, and the hybrid—and provide thumbnail career sketches of various exemplars.

Dogan and Pahre's thesis is provocative and they provide a real service by offering a sciencewide panorama against which one can appraise the goings-on within one's own discipline and department. Still, the evidence provided is more fragmentary and anecdotal than systematic; and I found my initial excitement weakening following introduction of the main points. The authors could have helped their case had they compressed their ideas into a thinner volume and exercised more care in bolstering their views. For example, by asserting that Arthur Bentley was trying to advance a theory of interest groups (which he explicitly denied) and that Harold Lasswell crossed the boundary from law to make important contributions to political science (when the reverse is the case), Dogan and Pahre leave the impression that they were writing in great haste. In numerous other instances their recitation of examples of hybridization's successes is simply thin and creates an impression of erudition that gradually fades as one reads on. Defects notwithstanding, the book makes an important point worth keeping in mind as the social sciences continue to fragment, evolve, and recombine.

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The Hellenistic Stoa: Political Thought and Action. By Andrew Erskine. Ithaca: Cornell University Press, 1990. 233p. $37.50.

The political thought of the Stoic school in the Hellenistic period (322 B.C.–29 B.C.) has often been regarded as an important stage in the development of Western democratic theory. Judging largely on the basis of alleged differences between the moral and political theory of Cicero at the end of this period and Aristotle at the beginning, the Carlyles in their influential study of medieval political thought pointed to this period of Stoicism for the seeds and first buds of democratic constitutionalism. More recently, the emphasis on a "critical turn" in this period has been countered by seeing in Cicero's thinking more continuity with classical political philosophy in its Socratic, Platonic and Aristotelian forms. Andrew Erskine's *Hellenistic Stoa* reconstructs the political thought of the first Stoics and traces its development through this time in a way that lends some support to each of those prior interpretations.

For Erskine, radical democratic ideals forged in opposition to the thought of Plato and Aristotle characterize the political thinking of Zeno the Stoic founder and the early Stoa; but through the Hellenistic period these ideals come to be transformed within the school in the direction of a more class-based, conservative thinking. Thus, Stoic thinkers of the second century, like Panaetius, are seen through the texts of Cicero to be more inclined to cite and rely on Plato, Aristotle, and their disciples than on the likes of such Stoic luminaries as Zeno, Cleanthes, and Chrysippus. Erskine's treatment of this development is not without the cast of its being a "thermidor" or even a revolution betrayed.

In tracing the development of Stoic political thought, Erskine is especially concerned not only to free his readers from the common view of Stoicism as primarily a personal and a political philosophy, a view formed in the light of such extant later Stoic writings as those of Epictetus and Seneca but also to show that the Stoicism of the Hellenistic period sought to influence, and was influenced by, the political contentions of the time. Thus Erskine's reconstructions of Stoic views on equality, political participation, property rights, and other basic concepts of political theory is done in tandem, with chapters speculating on the interaction of Stoic thought first with third-century Athenian and Spartan political struggles and then with the second- and first-century Gracchan constitutional crises in Rome and Roman efforts to justify its empire.

Erskine's reasonable concern to treat political thought in its relationship to "its social and political environment" turns out, however, to entail an almost exclusive emphasis on political philosophy's being a "response to circumstances," being "conditioned by the form of . . . [the philosopher's] society, its values and its beliefs" (pp. 2–3). What seems to get lost with such an emphasis is attention to how an idea may fit in the context of a given writing. Thus, the argument and intent of the whole of Cicero's elaborately constructed *De Republica* is not utilized when Erskine explains the raison d'être in that work of the arguments against justice by Philus and of the well-known speech of Laelius on natural law. For him, both of these are primarily related to a philosophy of empire required when Cicero wrote. A much more important loss to this analysis stemming from the emphasis on the social and political environment of thought is the failure to inquire, at least openly, about the soundness and adequacy of the political thinking explored. Thus the Roman "establishment" is seen welcoming and encouraging the developments in Greek Stoicism that "were least threatening to their own society or could be made most compatible" (p. 209). Erskine chooses not to ask directly whether early Stoic thought came to be transformed chiefly because it was (as Panaetius and Cicero thought) poor political theory, inadequate to the human condition, and not at all marked by the subtlety and profundity of the Socratic tradition.

One reason Erskine is not found doing much overt, critical assessment of the ideas he is recounting may be the scant and indirect sources he has available for reconstructing Stoic political thought. This has always been the problem for getting at Hellenistic Stoicism; and early in the book Erskine reminds us that his reconstruction is to be done from Cicero and even later accounts in Plutarch and Diogenes Laertius. The scope of Erskine's reconstruction and his assurance in it do not suggest that humility in face of limited materials keeps him from critically assessing that "democratic moment" in early Stoicism. It seems, rather, that Erskine's silence is related to another form of his assurance, namely, his conviction of the simple superiority of early Stoic thought to that of Plato and Aristotle. Much of this book is given to differentiating early Stoicism from the theories of Plato and Aristotle. This is not a bright aspect of the book; for often, those theories are not presented with the understanding and fairness requisite for analysis of classical political theory. Perhaps the most egregious example of such a "miscomparison" is found in his claim that only with the Stoic treatment of justice do we get a conception that transcends "conventional social units" and provides a measure for all cities (pp. 114-15). This makes the nature-convention distinction a Stoic discovery!

WALTER NICGORSKI

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The Spirit of the Constitution: Five Conversations. Edited by Robert A. Goldwin and Robert A. Licht. Washington: American Enterprise Institute Press, 1990. 114p. $27.50 cloth, $14.95 paper.

The Federalist Papers and the New Institutionalism. Edited by Bernard Grofman and Donald Wittman. New York: Agathon, 1989. 300p. $42.00 cloth, $18.00 paper.

One United People: The Federalist Papers and the National Idea. By Edward Millican. Lexington: University of Kentucky Press, 1990. 277p. $29.00.

These volumes explore the values, assumptions, and goals of the founding period that not only undergird our constitutional order but provide it a sense of direction as well. The Goldwin-Licht volume—the most far-ranging and stimulating of the three—records the give-and-take between political scientists, jurists, and journalists (some 30 in all) "on the great objects of the framers' Constitution" with the end of delving "into the spirit of the framers' deliberations at the time of the Constitution." The Millican and Grofman-Whitman works focus—albeit from significantly different perspectives—primarily on *The Federalist* and more prosaic matters such as Publius' views on the distribution of powers between the national and state governments, the relationships between institutions, and the character of republican government.

The overarching concern for Goldwin and Licht is whether the "spirit" or "unspoken principles" of the Constitution can be discovered in its "federal character, commerce, democracy, liberty and equality, religion, or some combination of them" (p. x). What emerges from the five "conversations" (one for each of these themes) are fundamentally incompatible views on issues critical to any assessment of the Constitution's "spirit." For instance, while most of the participants feel that the Constitution embodies a "democratic" spirit, significant differences emerge concerning the source and nature of that "spirit." Not the least of these centered on the degree to which the "letter of the Constitution"—incompatible with, and even hostile to, direct or plebiscitary democracy—can be viewed as evidence of the founders' distrust of the people and whether the "spirit" of democracy emanates not from the text of the Constitution itself but from viewing it in light of the principles of the Declaration of Independence. Moreover, though these "conversations" reveal a consensus concerning the centrality of the Declaration of Independence to the tradition, the discussion on "liberty and equality" reveals fundamental disagreement among the participants concerning the "spirit" of the Declaration, particularly with regard to its "equality" clause.

These and other differences concerning the nature of the Constitution are concretely reflected throughout in the form of debates over contemporary issues involving, among other things, the need for constitutional reform; whether affirmative action and quotas are in keeping with the "spirit" of the Constitution; and the proper relationship between church and state. For this reason, the work is highly suitable for courses in the U.S. field, particularly ones that seek to relate political theory and practice.

Millican's work is narrow in scope, though its aims are highly revisionary. At the outset he writes that he wants to show that the authors of *The Federalist* hold "an essentially nationalistic viewpoint"; that they "presuppose the existence of a tangible, and paramount, American national interest," which "can be upheld only by an energetic and truly sovereign central regime" (p. 1). From this basically tenable thesis, he proceeds to render Publius' beliefs and assumptions almost indistinguishable from those of the early-twentieth-century Progressives such as Herbert Croly. Publius, he contends, envisioned a "guided democracy" not unlike that undertaken by nations "in the last two centuries" that have experienced similar "national revolution[s]." (Nkrumah's efforts in Ghana are held up as example in the last chapter.) Consonant with his thesis, Millican argues that Publius ("particularly Madison"), contrary to what modern pluralists hold, was no "advocate of political pluralism." In fact, he maintains, Madison "did not like special-interest groups, which he called 'factions', and he wanted them removed from the governmental process, not made part and parcel of it" (p. 13).

Millican's work is marred by serious procedural and substantive shortcomings. The book is too long by a half, largely because he surveys each of the 85 essays in order, save for detours necessitated by his separate treatment of Hamilton and Madison. And why Hamilton and Madison should be treated separately is a mystery, since Millican at numerous points rejects the "split personality" thesis. Substantively speaking, Millican's analysis downplays or entirely ignores evidence contrary to his thesis or considerations that would significantly modify it. In this category would fall Publius' frequent and relatively clear statements of the ends or goals of the federal government that even from Hamilton's pen, fall far short of anything contemplated by the modern advocates of "guided democracy." Likewise, Publius' teachings regarding interests, factions, and parties do contain an important normative dimension that Millican overlooks or misconstrues, namely, their existence is a function of liberty, a value more dearly cherished by Publius than either republicanism or a strong national government.

The 14 articles that comprise the Grofman-Whitman volume will be of interest to students of *The Federalist* mainly because of their approaches that utilize the tools of "new institutionalism," that is, "tools derived from microeconomics, game theory, and social choice of the effect of decision-making rules and institutional structure on outcomes" (p. 1). One or more of these tools is employed to explore and analyze various concepts and propositions central to Publius' thought. Among the major areas explored are Publius' conception of republicanism and its conformance with "public choice" theory; the effect of institutions and constitutional rules on behavior; the comparative powers of the various institutions to impose their will; and the costs and benefits (efficiency and stability) of the constitutional institutions and procedures.

The problems inherent to new institutionalism analyses are recognized at various points in this work but especially in Mark P. Petracca's contribution. Though confined to the institutional power studies, his critique points to potential difficulties in all studies that employ the tools of new institutionalism. There is, first, the question of whether the methodologies used are appropriate to the phenomena under examination—whether, for example, the methods used "to calculate a power index for the President, the House, and the Senate" can accommodate to the intricacies and complexities of the lawmaking process wherein constitutionally independent institutions interact with one another (p. 164). A second difficulty arises when propositions or concepts selected for analysis are "taken out of appropriate historical" or textual "context." Clearly, to the extent that this is the case, the analysis will hardly lead to an understanding of Publius' thoughts. In this respect, there is no question that many of the contributors would have benefited from greater exposure to the more traditional analyses of *The Federalist.*

While providing a new way of looking at *The Federalist* that will, no doubt, prove heuristic for some, this volume does not substantially advance our understanding of its underlying theory. What is acknowledged in the preface, however, is the debt that public choice theorists owe Publius, namely, an awareness of "the role of deliberation and rational persuasion, a concern for justice and the search for the public good, and a respect for civic virtue and civic education" (p. xv).

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Book Reviews: Political Theory

The Political Power of Economic Ideas: Keynesianism across Nations. Edited by Peter A. Hall. Princeton: Princeton University Press, 1989. 406p. $49.50 cloth, $16.60 paper.

The Spread of Economic Ideas. Edited by David Colander and A. W. Coats. Cambridge: Cambridge University Press, 1989. 262p. $34.50 cloth.

How do scientific ideas affect politics? A priori, one is drawn to some variant of Thomas Kuhn's vision of scientific progress: "normal" science fails in practical application; theorists endeavor desperately to explain away the contradictions or to tinker with the old paradigm; one or more new theories are proposed, dividing the scientific community; a new consensus forms, the few hold-outs are ostracized; and the reunited professionals sell their revised understanding to the policy makers. The cardinal instance of such a process in our own century, most would assume, was the triumph of Keynesian economics: orthodox theory having led policy makers to social and political disaster, Keynes's theory of underconsumption and his prescription of anticyclical fiscal policy won over first the academic and then the policy-making communities, until even Richard Nixon could proclaim that "We are all Keynesians now."

The excellently focused and carefully researched studies in the volume edited by Peter Hall show persuasively that nothing could be farther from the truth. In France, Germany, and Japan, Keynes remained almost unknown even in the academic community in the 1930s. Among Italian economists, the *General Theory* was widely read in those years but even more widely rejected. Scandinavian theorists, although they endorsed demand management, saw much of Keynes's work as inapplicable to their small, open, export-oriented economies. After World War II, despite the missionary efforts of Marshall Plan officials and U.S. economists, policy remained obduratedly un-Keynesian in Germany, Japan, Italy, and Finland; even Sweden and Norway practiced only what Jukka Pekkarinen calls, in a contribution that stands out even in this superb company, "a specific form of Keynesianism" that incorporated both "a vision of the supply side" and "tight fiscal policy." In France, Pierre Rosanvallon demonstrates, Keynesianism triumphed after the war only in policy and among technocrats; the academic economists remained rigidly orthodox until the late 1960s. Only in terminology and in national income accounting, Hall observes, do Keynesian ideas appear to have penetrated almost universally.

In Britain and the United States, history did appear to follow the Kuhnian pattern, albeit (as Margaret Weir contends) along slightly different paths: while rapidly winning academic hegemony in both countries, Keynesian ideas penetrated the porous U.S. government more quickly and less durably than they did the central, insulated, and powerful British Treasury.

If, as Hall concludes, "One of the most striking findings of this study is the degree to which Keynes's ideas about demand management were resisted or ignored in many nations" (p. 367), the chapters by Walter S. Salant and Bradford A. Lee demonstrate almost as strikingly the extent to which "proto-Keynesian" proposals for the stimulation of demand had been put forward not merely by "cranks" and "scribblers" but by eminent economists (including Keynes himself), only to be ignored. The coherent theoretical basis that the *General Theory* provided proved, at least in the democracies, a necessary if not sufficient condition for turning such proposals into policy.

In his insightful concluding chapter, Hall offers three principal explanations for the frequent failure of Keynesianism as policy. First, the prescriptions were in fact suited only to the closed industrial economies; hence, they were received "coolly" in export-dependent or significantly agricultural states. Second, Keynesian doctrine found a ready audience only where high unemployment was conjoint with an appeal for working-class support. Where policy makers confronted instead inflation (early postwar Japan and Germany) or overemployment (Italy and Germany in the 1950s) or where bourgeois and traditionally conservative parties dominated (Finland, Switzerland), Keynesian proposals fell on barren soil. Third, governmental capacity mattered: demand management held greater appeal where a powerful bureaucracy, a large state budget, and well-developed techniques of public finance made it easy.

Hall correctly summarizes his collaborators' principal findings, but he fails to grasp their full significance. Instead of the international disciplinary monopoly that Kuhnian science presupposed, we find in this signal case an intensely competitive market for ideas, in which policy makers buy with a discerning sense of fit, quality, and need. Far from being the "slaves of some defunct economist" that Keynes himself imagined such "practical men" (and, even then, women) to be, politicians and civil servants adopt the doctrines that credibly promise to solve, preferably with tools already at hand, the problems they actually face. In assessing credibility, professional legitimacy (the "right" degrees, scholarly consensus, prestigious journals and presses) plays an important but (as the interwar German, Japanese, and Italian cases reveal) not essential role.

The conclusion (much against the initial grain of the project's organizers, one suspects) is that the political power of economic "ideas" (and, more to the point, of economics as a discipline) is extremely limited. For all of Hall's efforts to convince us that "Keynesian ideas did not simply reflect . . . material conditions" but "had a good deal of independent force over circumstances," the individual studies drive one inexorably to the opposite belief.

Despite the centrality of the Keynesian "revolution" in modern economics, one could hope a broader study of how economic ideas are diffused might reestablish their importance. The volume edited by Colander and Coats, despite promising something of the kind in its title, *The Spread of Economic Ideas,* is a work of rumination rather than research. Great and semigreat economists, together with some editors, foundation officers, and economic journalists, relate their impressions about how economic ideas are spread—to other economists, to the lay public, to policy makers, and by or through funding agencies. Charles Kindleberger tells us what publications he regularly reads, Robert Solow bemoans the press's inability to get government prizes for good economic ideas. It is all good fun, but it has the flavor and the cumulative impact of a series of after-dinner speeches.

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The Necessity of Choice: Nineteenth-Century Political Thought. By Louis Hartz. Edited by Paul Roazen. New Brunswick, NJ: Transaction, 1990. 215p. $24.95.

The Shaping of Liberal Politics in Revolutionary France: A Comparative Perspective. By Anne Sa'adah. Princeton: Princeton University Press, 1990. 248p. $32.50.

The best—or at least the most serious—political thinking is often said to occur when a society is undergoing a severe social conflict that raises questions about its own legitimacy. Both of these books confirm the saying but not without some qualifications. Each is interested in how much political ideals can count in situations where other more structural causes are also at work. The French Revolution, the subject of Sa'adah's study and the opening essay in Hartz's book, is seen as a test case of just how much (or how little) ideas, especially liberalism, count when a society needs to redefine itself around a new conception of legitimacy. Their judgments on this score are broadly similar. Hartz sees nineteenth-century political thought as a failure because it evaded rather than solved the problem of how to remain, under changing circumstances, faithful to the Enlightenment ideal of liberty. Sa'adah labels the French Revolution as a "failure" insofar as it was unable to translate the liberal fear of political power into some more positive ideology which could serve as the foundation for a reconstructed community. But Hartz's criticism is by far the harshest. Since he assigns ideas far greater independence than Sa'adah, he is inclined to indict much of nineteenth-century political thought for preferring either to flee the real world or embrace it uncritically, the choice of the French philosophes and Burke, respectively. Because Sa'adah sees ideas and ideology as mediating between a multiple number of structural forces channeling events, her criticism is more nuanced, distinguishing, for example, between what was essentially a failure of nerve on the part of leading figures in the revolution and moments when historical circumstances overwhelmed all participants. Sa'adah's judgments are reinforced and importantly expanded by the contrasts she draws between the French case and the English and U.S. revolutionary settlements where a moderate liberal politics triumphed.

The Necessity of Choice actually consists of the lectures that constituted Hartz's graduate course on the nineteenth century at Harvard. Hartz had a clear and generous vision of the potential for political thought, sufficient, I think, to justify the decision of one of his former students, Paul Roazen, to compile and prepare these lectures. Nonetheless, lectures, even superb ones as these surely were, can sound stilted when transcribed rather than delivered—a problem exacerbated by the fact that none of Hartz's notes for the course could be found. Moreover, Hartz, not unlike many of us, often gives very cursory treatment to a number of major thinkers in order to keep things moving. More generously, one might say that Hartz had his own thesis to argue and that he could afford briefly to assemble thinkers who helped him make his points. The "necessity of choice" that Roazen uses to characterize that thesis refers to Hartz's conviction that every society must choose how, given local circumstances, to realize the Enlightenment ideal of liberty, to unite theory and practice. This conviction controls his reading of nineteenth-century political thinkers. Coming in for hard treatment are those who ignore the "fact of society," preferring abstract theorizing to any more practical political sociology (e.g., the French philosophes and physiocrats). Equally criticized are thinkers who embrace a brute empiricism and thus failed to see that political ideals and aspirations were also "social facts." Treated more favorably are Rousseau, J. S. Mill, Constant, and even Bentham for seeing that society is problematic and that questions about the free state and the free society are the same question. Hartz's conclusion that the nineteenth century failed to answer the questions posed for it by the French Revolution is made more ominous by the "solution" with which he begins these lectures. Anticipating a point critical to Sa'adah's book, Hartz views the French Revolution's appeal to an unlimited state as the price paid for bad political thinking. And he had no illusions about the incompatibility of this and other statist expressions of human solidarity with a free society.

Anne Sa'adah's controversial thesis, ably defended, is that the French Revolution began as a revolt inspired by liberal values. In this connection, she has sensible things to say about Rousseau and the appropriation of his ideas by the leaders of the revolution. The regime's opponents, she argues, were committed to a system of unstable ideas. First, they were violently opposed to the private world of privilege and the corruption of the public realm that were hallmarks of the Old Regime. That opposition engendered a deep suspicion of political power and any settled system of authority, a suspicion that corroded all efforts at coalition building on behalf of a stable political settlement. This exaggerated liberal fear of power was compounded by an equally exaggerated set of expectations about the possibility for a political community defined by a sense of moral unanimity. To square this circle, Sa'adah argues that the French Revolution practiced a "politics of exclusion" intended to distinguish "friends from enemies" (p. 14), a choice that effectively isolated all moderates and defeated all attempts to redefine the political community around a new conception of legitimacy. Jacobinism filled the resulting ideological vacuum by making "the guillotine, rather than vehement speeches or parliamentary turnover, the accepted agent of political exclusion" (p. 179).

For a number of reasons—the nature of the prerevolutionary regime, the fact that all major groups in France were entangled in the Old Regime, the absence of any common discourse that might have moderated the argument between the Regime's supporters and opponents—Sa'adah argues that the French "revolutionary opportunity" seriously complicated the chances for a stable political settlement. She does not, however, spare its leading figures for a failure of nerve at critical junctures that closely corresponds to what Louis Hartz saw as an act of intellectual dishonesty.

In *The Liberal Tradition in America* (1955), his earlier and justifiably famous book, Hartz argued that the consensus among the U.S. public about key (Lockean) political values forestalled any serious political thinking in the United States. Sa'adah's book, although it does not clearly endorse this point, invites reconsideration of the merits of a liberal settlement in which politics is conducted in a less intense key. She attributes the more stable and favorable political course of the English and U.S. revolutions to many factors. But one that stands out is the existence of what might be termed a constraining discourse. In England and America, she writes, "revolutionaries and their opponents argued—the verb is deliberate, for they did indeed argue, in public and at great length—in a common language, around a shared set of broad ideals" (p. 35). Sa'adah allows us to entertain the notion that Hartz mistook a stable model of liberal politics for the absence of a meaningful politics. And so far as I can tell, the publication of these lectures by Hartz is more likely to support rather than to argue against this possibility.

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The Origins of Citizenship in Ancient Athens. By Philip Brook Manville. Princeton: Princeton University Press, 1990. 265p. $35.00.

The Greek Discovery of Politics. By Christian Meier. Translated by David McLintock. Cambridge: Harvard University Press, 1990. 305p. $32.50.

The Tragedy of Political Theory: The Road Not Taken. By J. Peter Euben. Princeton: Princeton University Press, 1990. 314p. $45.00 cloth, $14.95 paper.

The study of Athenian democracy has always been a surrogate battlefield for the waging of contemporary intellectual and political debates. This was as true for Machiavelli and Rousseau, both of whom hoped to revive ancient civic virtue, as it was for the U.S. Federalists, who wanted their fellow citizens to avoid the instability and "tyranny of the majority" that vexed the classical polis. In recent years political theorists have focused on a different set of questions: To what extent does the classical polis provide us with a viable alternative to the dominant liberal and Marxist perspectives? In particular, to what extent does the polis, in contrast to liberal and Marxist regimes, constitute a model of a purely public ("political") form of life that could liberate modern man from the grip of unintentional private ("social") forces?

Philip Brook Manville's analysis of the early history of Athenian democracy has important implications for this debate, even if it does not address it explicitly. Manville presents an eminently readable account of the urbanization of the Attic population and the development of democratic culture, laws, and institutions from the time of Homer to the reforms of Cleisthenes. Using the methodology of comparative anthropology, he builds a case that a radical increase in population and a competition for arable soil led to a land tenure crisis to which the reforms of Solon and Cleisthenes were the response.

Manville's comparative anthropological approach, while at times providing a novel perspective, has distinct limitations. By grounding explanation in cross-case similarities, it inevitably downplays the uniqueness of the Athenian experiment. But it is often questions of *difference*, not similarity, that fascinate us the most about the Athenian polis. For example, Manville himself notes that the crises of the early sixth century led to the formation of tyrannies in many Greek cities. "Solon," he writes, "might have done the same," especially in light of his military experience and skills. Instead he "chose to mediate the problems of his day through political means" (p. 125). Many crucial transitions such as this, precisely because they are unique to Athens and especially when they involve political "choice," remain outside of the book's explanatory framework. Manville's comparative methodology assumes that the most important variables are broad, unintentional social processes operating "over the heads" of actors—precisely the variables over which the Athenians sought to gain mastery with a distinctly political way of life. Manville decries the "decline of civic consciousness" in the modern world; but if the political is embedded in the social, it is difficult to see why he thinks this matters.

If Manville downplays the political, Christian Meier downplays the social. In his first two chapters Meier presents a theoretical analysis of the concept of the political that has distinctly Arendtian implications. To Meier, the Greeks were "the one political civilization among all the others the world has known" (p. 7). His book tells the story of the series of developments in preclassical Athens whereby "communal life was wrested from the control of self-motivating processes and made subject to political action." In the modern world, in contrast, "political action itself has become overlaid with processes by which we ourselves—and our identity—appear to be 'motorized'" (p. 6). His analysis is grounded in the careful scholarship and perceptive analysis that have long been associated with the tradition of German classical studies. For instance, he makes a persuasive argument that Aeschylus' *Eumenides*, performed in 458, must be read as a direct commentary on the clash between the nobility and the demos that came to a head with the abolition of the Areopagus by Ephialtes in 462/612. But in the end, Meier subordinates historical particularity to metaphysical teleology: the Greeks did not manage or contain economic crises or class tensions so much as they invented a democratic way of life that allowed them to transcend these purely "social" forces.

Peter Euben's study of classical tragedy and philosophy is a masterfully conceived work that explores the ambiguous terrain that separates the approaches of Manville and Meier. While Euben shares Meier's Arendtian admiration for Athenian politics, he balances this with a solid appreciation of Foucault's insight that all discourse, including the discourse of Athenian democracy, can function to privilege and deprivilege, normalize and abnormalize, empower and disempower. Euben seeks to use the Greeks to construct an approach to democracy that balances the "humanist" or "Enlightenment" passion for rationality and order against the "poststructuralist" or "postmodernist" drive to undermine stable foundations wherever they can be found.

To Euben, Greek drama functioned as an institutionalized mechanism to maintain this balance. Like Martha Nussbaum, Euben sees ambiguity and conflicting justice claims as central not just to particular tragedies but to tragedy itself as a sustained genre. Euben writes:

> Part of the city's order, [tragedy] "problematized" the idea of order, contesting as it constructed, challenging what it helped to sustain, and presenting transgression, paradoxes, and archaisms to disrupt civic teleologies which it eventually reaffirmed. Tragedy played [quoting Foucault] 'outside the ordered table of resemblances,' creating internal disturbances, strategically questioning the political and intellectual tradition with tools that tradition supplied, even providing competing versions of culture's constituting myths and narratives. (p. 29)

Euben builds this case with challenging readings of Aeschylus' *Oresteia*, Sophocles' *Oedipus*, and Euripides' *Bacchae*, to which he juxtaposes interpretations of Thucydides' account of the Corcyrean Revolution, Plato's *Apology*, and the *Republic*, respectively. By disrupting the canonical ordering of Greek texts, he underscores his point that the messages of these texts are profoundly ironic and self-referential. He concludes with a subtle analysis of Pynchon's *The Crying of Lot 49.* To Pynchon, and to Euben, the best hope for contemporary democracy lies in a revival of the classical insight that we live on the cusp between the actionable and the fated—that both clarity and mystery, modernist potential and postmodernist limitations, lie at the core of the human condition.

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Marxism, Pluralism, and Beyond: Classic Debates and New Departures. By Gregor McLennan. Cambridge, MA: Basil Blackwell, 1990. 304p. $49.95 cloth, $18.95 paper.

The Politics of Subversion: A Manifesto for the Twenty-first Century. By Antonio Negri. Cambridge, MA: Basil Blackwell, 1989. 232p. $39.95.

What, if anything, lies beyond Marxism and pluralism? These books intersect at this question, to which their authors provide very different answers.

For McLennan, allegedly new social theories—for example, radical democracy (Laclau and Mouffe) and postmodernism (Deleuze and Foucault)—participate in an "unfinished dialogue" with their more orthodox ancestors (p. 274). By claiming to be other and more than "mere pluralism," these theories concede much to the Marxist tradition. Yet the commitment to philosophical and political pluralism with which those concessions are combined challenges the scientific status of Marxism (pp. 157–59). The result—coexistence and contradiction of Marxism and pluralism—McLennan finds both necessary and proper. He envisions an "'eternal' oscillation . . . between the modalities of integration and diversity" (p. 263). His primary purpose here is to "encourage the proper *attitude*" toward that interaction (p. 259). The goal is not to establish the superiority of either side nor to condemn their mutual failures but to use the "armory of analytic tools" that both approaches still provide (p. 262). He refers to "contrasting appeals of different vocabularies" (his concession to postmodernism) as opposed to "clash[es] of rigid and always incompatible conceptual boxes" (p. 274). The heuristic and substantive strength of this attitude is, he argues, evident in attempts to understand emerging events in Eastern Europe or the subordination of neocorporatist states to global capitalist interests.

Though I have reservations about his "modest eclecticism," this is a very valuable book. I found the first part, where McLennan analyzes the opposition and evolution of Marxism and pluralism, the most helpful. Although they are familiar, McLennan presents the problems posed for Marxist orthodoxy by the relative autonomy of the state and the absence of class consciousness extremely well. Less familiar is the development from conventional to critical pluralism. McLennan's discussion of it effectively undermines "leftist" attempts to construct a pluralist straw man. Still, the "modest eclecticism" McLennan defends is ultimately a "pluralist Marxism, not a critical pluralism" (p. 262). It is Marxism, he claims, that provides the structural benchmarks for assessing the value of pluralist investigations. Such a choice (really, emphasis) requires further defense. However, McLennan warns readers that establishing this theoretical position is not his intent here (p. x).

In an otherwise extraordinarily comprehensive review, McLennan does not discuss Negri's work. This, as Moulier's introduction to *The Politics of Subversion* points out, is not unusual. English-speaking audiences have had limited access to Negri and to the *autonomia* movement from which he arises. That is unfortunate, since Negri's work begins to challenge McLennan's arguments against a "beyond." I will try to suggest how.

According to Negri, "high capitalism" has achieved socialism; exploitation is now a social and global phenomenon (p. 182). (Negri argues that Marx anticipated this with his references to the "real subsumption" of capital. For this reason, Moulier's introduction deems Negri a postsocialist Marxist and "one of the rarest tree-surgeons working on the old trunk of Western Marxism" [p. 3].) However—and here Negri engages in a Hegelian-inspired inversion—"high capitalism" is the result of workers' power. It is socialized workers who achieve the social cooperation necessary for capitalist production to proceed (p. 81). In this lies a basic antagonism and the possibility of subversion: workers can reappropriate social control. But reappropriation cannot occur through a proletarian party committed to a labor utopia. "Achieved socialism"—capitalist and communist—has discredited these options. Instead, Negri advocates the antagonistic production of subjectivity. This attack has two prongs, a politics of autonomy and autonomy of the subject (p. 147). The latter, called "autovalorization" and manifested in the "refusal to work," is clearer than the former. Its roots, he admits, are pluralist. The former requires an ontological and institutional reorientation, which Negri distinguishes from pluralist "indeterminacy." Unfortunately, Negri leaves those reorientations undeveloped; they remain problems and challenges. Ontologically, he alludes to a subterranean reconstruction of workers' consciousness to be manifested in collective affirmations of the "right not to work" that technology now makes possible (p. 217). To institutionalize such consciousness, "what is needed is a radically democratic type of subversion where organizational forms have the impact of Leninism and the freedom of autonomy" (p. 167). In other words, "proletarian self-valorization" must converge with "open institutionality" (p. 190). (Negri interestingly regards postmodernism as an allusion to the "scientific determination of new subjects," but in a mystified or ideological form [p. 206].)

Negri certainly has not resolved the Marxist-pluralist debate. But does he simply reformulate it? Is his work another example of the "modest eclecticism" McLennan defends? Negri's vocabulary, with all its difficulty, is seemingly familiar. Yet while struggling with his prose, I was reminded of Althusser's concept of an "epistemological break." To declare the Marxist-pluralist debate "eternal," as McLennan does, forecloses that possibility. "Modest eclecticism" may characterize the near future of social theory. But perhaps that is only because social theorists are groping for words, for answers to questions they cannot yet ask. It is my sense that Negri's work, and postmodernism, anticipate much more than "modest eclecticism."

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A Government of Laws: Political Theory, Religion, and the American Founding. By Ellis Sandoz. Baton Rouge: Louisiana State University Press, 1990. 259p. $37.50.

What were the foundations for the political ideas of the U.S. founders? Ellis Sandoz in *A Government of Laws* takes up this much-debated question in an attempt to "relate the political consensus of the American founding . . . to the heritage of politics, philosophy, and religion that consciously informs it" (p. xii). Sandoz's thesis is that the ultimate theoretical origins of the U.S. founding can be found in classical philosophy and Christianity (pp. 84, 100, 114, 118, 152). Drawing extensively from contemporary sources, Sandoz argues that the founders adapted this philosophical and religious tradition to the U.S. experience. This insightful book makes a positive contribution to our understanding of the role classical and religious ideas played in the U.S. founding; and it offers a compelling critique of analyses that highlight secular Enlightenment, liberal, and republican ideologies to explain America's heritage.

Sandoz begins his book with a discussion of the "existential truths" of human nature (p. 3), which he claims the founders articulated in their political ideology. The fundamental insight of both classical and Christian theory is the "tensional dimension of participatory reality" (p. 7), which Sandoz calls the *in-between reality:* "Man is neither brute nor is he God; he is something in-between. . . . He is capable of virtue and faith but inclined to sin and vice" (p. 9). In Christian theology, this tension is expressed in the twin doctrines of the Creation and the Fall. Individuals have dignity, equality, and worth because they are created in God's image. Because of human sin, however, people have lost the capacity to rise above the natural order and have communion with God. Humans are prideful, ambitious, self-interested creatures who know the good but cannot always seek it. The fall has not, however, impaired human capacity for natural justice and reason, both of which are divine traits of human nature (pp. 84, 145, 181–82).

Sandoz argues that these ideas shaped the political thoughts and behavior of the founders. For example, Sandoz demonstrates how the appeal to self-evident truths in the Declaration of Independence was grounded on the conviction that "human beings are created equal and endowed by their Creator with unalienable rights to life, liberty, and the pursuit of happiness" (pp. 208–9). This natural law teaching of classical and Christian philosophy provided the founders with a set of rights that were "logically and ontologically superior to the social and political orders" (p. 192). George III had perverted his powers by overstepping the limits imposed by higher law; and resistance to his tyranny became a sacred obligation and a natural right for Americans who believed that liberty was God-given. The human laws of George III were unjust because they conflicted with God's divine laws and were no longer binding on the Christian colonists, who knew that their ultimate allegiance belonged not to men but to God. These convictions, which Sandoz claims were pervasive, provided the legitimation necessary to sustain a revolutionary ideology (p. 159).

Sandoz proceeds to argue that these beliefs also shaped the new governmental structures formed by the founders after the revolution and the ineffectual government under the Articles of Confederation. The framers created an institutional design that reflected the classical and Christian philosophers' notion of human existence in the "in-between reality" (p. 179). The new government minimized the consequences of human sin while still providing the foundation for the natural rights of liberty and justice, which the founders believed to be the objective right of all persons. Nowhere is the influence of Christian thought more evident than in role of the separation of powers and system of checks and balances created by the framers to cope with the combative interests and passions of men. The Christian anthropology, which teaches that human beings are sinful, required a structural mechanism to counteract the destructive consequences of unchecked human passions; effective government could be made to minimize the impact of concupiscence. Sandoz also argues that the Bill of

Rights institutionalized the natural law teaching that individuals enjoyed rights independent of the government.

These insights make the book a helpful corrective to a debate which has largely ignored the importance of classical and Christian ideas to the founding, but it raises many questions that it fails adequately to address. For example, the author's main argument that ideas significantly shape actors' perceptions and behavior is not effectively defended against critics who contend that religious and philosophical ideas were simply used by the founders to justify positions they had reached by other means. Sandoz laudably admits that the founders made use of multiple sources in framing the new Constitution, but that begs the question of when classical and religious claims were autonomously important. Although he demonstrates the pervasiveness of religious practice and belief in the United States at the time of the founding, Sandoz never clarifies why millenarian and natural rights arguments were more persuasive during the revolutionary period than during the framing of the Constitution. Is it a coincidence that human sin became a more compelling religious theme for the founders as they struggled to minimize the expectations of what political institutions could provide, or is it possible that a revolution and constitution require different kinds of thinking? If so, what happens to the autonomous role of religious and philosophical ideas?

Even if the central thesis is accepted, it is not apparent that classical and Christian ideas influenced the founders in the way Sandoz asserts. The author states, without demonstrating, that the U.S. public shared a basic agreement on the principles of Christianity, a claim that ignores the genuine difference among denominations and between the elite and the masses at the time of the U.S. founding. The framers may have reconciled natural law and Christian anthropology, but it is not apparent that the Calvinist colonists, who believed that a repercussion of the Fall was that human reason could not be a base of natural justice, would have. Finally, the author hints that a basic philosophical and religious agreement is an essential element for the moral foundations of a society, which opens the provocative issue of what happens to the United States in the intervening centuries when the natural law foundations of the Constitution are abandoned.

The book, however, is well worth reading. Sandoz offers a compelling case that religious and classical ideas were important for the U.S. founding, and it deserves the attention of scholars interested in U.S. political thought.

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Political Returns: Irony in Politics and Theory from Plato to the Antinuclear Movement. By John Evan Seery. Boulder: Westview, 1990. 384p. $39.95 cloth, $14.95 paper.

The instructor of political theory perhaps faces no greater challenge in the modern classroom than that posed by the demand for relevance. Questions like "So what?" and "Why should I care?" haunt discussions about justice and goodness with growing intensity, and nowhere is this modern skepticism more poignant than in the threat of nuclear annihilation. Remarkably, it is exactly this expression of hopelessness that John Evan Seery has chosen to be the touchstone for his theory of "ironic political communitarianism" (p. 20)—a theory with which he hopes, paradoxically, to open up a space for the relevance of politics in the modern world.

The practical intention of *Political Returns* is "to present a political theory of the antinuclear movement" (p. 15), but Seery's project actually makes a much more fundamental claim about the nature of modern politics. Calling upon a tradition of "irony" in politics that he traces back to Plato and the Greek tragedians, Seery constructs an argument for community that is built right on top of the fact of human mortality. His basic insight is that "the affirmation of others is to be attained only . . . through an acceptance of a pretense . . . that caring about another human being matters at all in view of human finitude" (p. 4). The fact of death makes impossible any ultimate justification for living and achieving; and politics, as the art of living together, falls under the same curse. In Seery's words, "to choose to affirm human community in the face of human mortality, . . . to be dedicated when one is also deeply doubtful . . . suggests a philosophical stance that involves a double perspective on things. . . . That double perspective corresponds closely . . . to what can be properly called an ironic outlook" (p. 343). The irony is to be found in the creation of a real political relationship, a real community, by means of an acknowledged pretense—a "big lie" (p. 115).

In the modern context, it is skepticism that opens the door to this ironic politics, for in our individual confrontations with the emptiness of the world, we share the experience of nihilism and—if we wish to go on living at all—the need for a new rationale to carry us forward into action: "Ironic theorists make a method, construct a creative conceit, out of [the] very awareness of their own limitations and the preciousness of their enterprise" (p. 345). For the antinuclear movement, the conceit is the "big lie" that diverse people should be able to live together without the threat of nuclear conflict. It is a lie because we modern skeptics know that there is "no 'natural' reason" why human beings should live together in peace (p. 341). But the tangible result of this lie is the real community that forms around it; and the proof of the truth of the lie is, ironically, the existence of the heterogeneous community of peaceful individuals that is the antinuclear movement itself: "The last twist of irony . . . is to turn its own skepticism 'backwards' upon itself, in order to affirm existence" (p. 344).

Seery's argument eloquently weaves its way through the history of philosophy, focusing on the role of irony in the work of a parade of well-known theorists. Sometimes his exposition is surprisingly innovative, revealing new ways of looking at ideas thought to be well understood. At other points though, the catalogue of names is too dense; and the result is an unsatisfying rat-a-tat of academic mind-bytes, probably left over from an otherwise well-revised dissertation. Two of the highest points of the book are a heartfelt critique of Sheldon Wolin's *Politics and Vision* (1960)—in which Seery criticizes the versions of communitarianism that rely too much on some "natural" or "visual" and not "theoretical" or "constructed" understanding of the relationship between politics and community (pp. 52-53)—and a fairly in-depth interpretive exploration of Plato's *Republic* as a "profoundly ironic work" (p. 72). While they certainly will not convince everyone, these chapters make for a lively debate.

The most nagging problem with Seery's ironic reading of Plato and the other texts he examines is perhaps best illustrated by what I take to be his farthest reach—his worst case. In his analysis of Marx, Seery calls upon the well-known conflict between Marx's Hegelian understanding of man as the maker of his own world (the so-called *homo faber* thesis) and the later Marx's determinism to make the almost Straussian claim that Marx didn't really mean what he said. Since irony, as Seery understands it, "places an especially heavy burden of interpretation on the reader" (p. 177) (in that the literal layer of the text cannot be taken completely at face value), the danger of reading too much between the lines becomes a real concern. Although Seery makes a determined effort to separate his ironic interpretation from the Straussians' wholesale inversion of the literal layer of a text (see pp. 82-84), statements like "Marx was deliberately overstating his case in using language that made his historical scheme seem inevitable" tend to blur the distinction. It may well be that Marx was "deliberately engaging in and consciously in control of his own [ironic] mythproduction" (p. 251); and Seery constructs an interesting case to that effect, using the preface to Marx's dissertation. But one must finally acknowledge that without at least some substantial literary evidence for an author's position, it is not productive to argue about whether he was "really" saying something else entirely.

Whether or not Seery succeeds in his reinterpretation of the classic texts of political philosophy, the theory of ironic politics that he offers is worthy of consideration in itself. It tells us once again what so many have before—that the world is essentially what we make it. Unlike the others though, this theory speaks directly to our own time. Perhaps even more importantly, it offers hope to our disbelieving age—without requiring that we first deny the skepticism that has become so familiar to the modern soul.

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Cultural Theory. By Michael Thompson, Richard Ellis, and Aaron Wildavsky. Boulder: Westview, 1990. 296p. $16.95 paper.

Following a stylish flurry of disciplinary excitement in the early 1960s, the concept of political culture was cast aside as purely classificatory, tautological, static, and bereft of explanatory power. Relegated thereafter to a peripheral place in intellectual histories of political science, it was otherwise dismissed as little more than a failed footnote to the emergence of the behavioral revolution and its immediate aftermath. Today, however, the concept has risen in phoenixlike fashion to engage the talents of prominent scholars. Those most responsible for this culturalist renaissance have included Harry Eckstein, Ronald Inglehart, and Aaron Wildavsky. The last of these is overseeing a series of publications on political culture, to which the present volume is an important contribution.

The series itself sees political cultures as describing persons who share values and beliefs legitimizing different ways of life. The study under review here centers attention upon ways of life that serve as basic tools for the dissection of people and political culture. Drawing explicitly on the work of the anthropologist Mary Douglas, the three coauthors employ her "grid-group" typology as the basis for categorizing and probing five ways of life. These are identified as hierarchy, egalitarianism, fatalism, individualism, and autonomy. In criticizing the customary disciplinary proclivity toward analytical dualism, the five ways of life are regarded as exhausting the possibilities in linking social relations with cultural biases. This insistence is termed the "impossibility theorem."

In the first of the book's three parts, there are extended definitional discussions too complex for treatment here. The approach is painstakingly thorough and internally logical while demanding close and laborious attention from the reader. Cultural theory emerges as a self-organizing system within which social relationships occur. The operative patterns and processes are outlined, although the subtleties of the argument are not easily accessible.

Cultural Theory proceeds in part 2 to argue the indispensability of functional explanation, doing so by exploring the works of past masters. Writing within the context of linking functions to ways of life, the coauthors begin with a chapter on Montesquieu, Comte, and Spencer; move through Durkheim, Marx, and Weber; and eventually close with Merton, Stinchcombe, and Elster. This sets the stage for a critique in part 3 of recent and contemporary classics by such scholars as Banfield, Pye, and Almond and Verba. These are uniformly thoughtful, if sometimes controversial, and merit close scrutiny in their own right.

For Thompson, Ellis, and Wildavsky, cultural theory has a legitimate claim to universality. They labor carefully to explain the boundaries for political culture, and in the concluding chapter return to their adaptation of Douglas's grid-group typology. On balance, they are persuasive in contending that cultural theory is relevant. They contribute to the resurrection of its conceptual potential for meaningful analysis, suggesting that empirical research may well buttress and enhance the relevance of political culture. Ultimately the coauthors close with the admonition that cultural theory should generate proposals for research projects "by which its validity can be tested, as well as . . . providing a position from which the major theoretical trends in social science can be surveyed, criticized, and compared" (p. 273).

Granted the undeniable strengths of this work, it remains true that the conceptual parameters of contemporary political culture research can still benefit from further clarification. Thompson, Ellis, and Wildavsky have provided us with an intellectual tour de force that is not yet easily translated into appropriate projects for inquiry and explanation. The stress on definitions has been salubrious, but less than all-encompassing. Certainly, the translation of sometimes esoteric conceptual refinements into more fundamental research design requires further attention by these and other scholars. At the same time, this volume makes a significant contribution in sharpening the focus of the spotlight on political culture and its conceptualization. Moreover, it illuminates the rooting of political preferences in political culture, which emerges as an unquestionably powerful and integrative conceptual construct.

JOHN D. MARTZ

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Book Reviews: Political Theory

Empire of Liberty: The Statecraft of Thomas Jefferson. By Robert W. Tucker and David C. Hendrickson. New York: Oxford University Press, 1990. 360p. $24.95.

Jefferson continues to stimulate commentary and analysis and is likely to do so far into the future, or at least as long as there is a United States and exploration into the intellectual roots of U.S. experience.

Tucker and Hendrickson have a clear-enough aim, thereby warranting a look at their work by anyone interested in Jefferson and the evolution of U.S. diplomacy. The book concentrates on Jefferson's statecraft and attempts to show the tension between democratic moral principles and the conduct of diplomacy. After all, Jefferson was not only the author of the Declaration of Independence and the original version of the Bill of Rights. He was also the United States' first secretary of state and was present at the creation of U.S. statecraft.

Tucker and Hendrickson's central thesis is that Jefferson's statecraft waivered between the two contradictory principles that the United States should provide an example of liberty and that the United States should be an active crusader in international affairs. As paralleling themes the authors frequently attempt to show ambiguities or contradictions between morality and reason of state and between principles and practice in Jefferson's thought and action. Finally they argue that these ambiguities are also reflected or shared in the subsequent conduct of U.S. foreign policy.

Tucker and Hendrickson create a picture of Jefferson somewhat different than the more self-consistent image conveyed in varying degrees by Gilbert Chinard, Merrill Peterson and Dumas Malone. Tucker and Hendrickson lean more toward the so-called darker side of Jefferson represented by the work of Leonard Levy. The strength of the book is in its comprehensiveness in touching on the main events in Jefferson's statecraft. However, the weakness of the book is in the subjective and also inductive manner in which Jefferson's actions are approached without a full analysis of the logic of Jefferson's commentary on both his theory and practice. If one works inductively from an interpretation of action to a search for meaning, self-consistency would be difficult to find even in the lives of saints and seers. Additionally, it is unwise to look for complete self-consistency in the actions of even the greatest political leaders unless one allows for personal growth, presence of factors beyond the leader's control and, finally, judgment calls in the face of incomplete information. These practical factors, when considered in the context of Jefferson's theoretical overview, can give us a more self-consistent Jefferson. It is possible to explain the inconsistencies of U.S. statecraft in terms of its Hamiltonian commercial interests, the rise of the un-Jeffersonian military-industrial complex, the reason of state and balancing of power by an Acheson, and the rollback crusade of a Dulles rather than seeing them as parallels of some deep ambiguities in Jeffersonian statecraft. The evidence provided by the authors themselves illustrates problems in their thesis. As an example, the authors couple Jefferson with Woodrow Wilson as great exemplars of moral leadership. Yet elsewhere the book contains clear evidence that Jefferson and Wilson were quite different on the crucial test of the principles of recognition of new governments. Wilson applied de jure standards while Jefferson opted for de facto recognition. Democratic legitimacy was not the only form of consent. This is not an example of reason of state but simply a recognition that not all societies had consented to a democratic system.

The major premises of Jefferson's statecraft are clear and consistent—the commitment to the constitutional system particularly the Bill of Rights, the commitment to international law rather than reason of state, the commitment to the freedom of navigation, the providing of moral leadership as a democratic country and a beacon of freedom, realistic aid to those who seek similar freedoms, patience and deliberation in negotiations, an avoidance of crusading, prudence in the realistic use of force in order to ensure success and yet avoidance of the long-term consequences of a large standing army. These premises are accompanied by some practical judgments—that an extensive geographical base is likely to avoid the parochialism of small classical democracies and that no more good should be attempted than people can bear. The former explains Jefferson's pursuit of the Louisiana Purchase. The latter explains Jefferson's conservatism and hesitation in pushing for rapid implementation of every constitutional principle and every right. While we may be critical of Jefferson's judgment in matters of practical timing, delay in implementation of principle does not necessarily imply a commitment to the doctrine of reason of state. There are of course some valid discrepancies in Jeffersonianism illustrated in Tucker and Hendrickson's work. But rather than the Louisiana Purchase, a more legitimate criticism can be leveled at the discrepancy between natural rights and the spurious consent involved in contractually separating American Indians from their property. While Jefferson's stance is not as unseemly and forcible as Jackson's Indian removal program, it illustrates the continuing clash between Anglo-American civilization and aboriginal rights and property. Jefferson is simply one among the long list of presidents who have ignored this generic flaw in the U.S. dream.

JOYOTPAUL CHAUDHURI

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Jean-Jacques Rousseau and the "Well-Ordered Society." By Maurizio Viroli. Translated by Derek Hanson. Cambridge: Cambridge University Press, 1988. 247p. $44.50.

Reading Rousseau in the Nuclear Age. By Grace G. Roosevelt. Philadelphia: Temple University Press, 1990. 275p. $44.95.

Jean-Jacques Rousseau and the "Well-Ordered Society" and *Reading Rousseau in the Nuclear Age* are two serious and in many ways complementary additions to recent Rousseau scholarship.

Viroli's study of political order in Rousseau's thought is one of a number of works in recent years to present a more nuanced, less radically democratic image of Rousseau than the one that is still perhaps most common. (Precedents in this regard include Judith Shklar's *Men and Citizens* [1969] and Hilail Gildin's *Rousseau's Social Contract* [1983].) Viroli's study takes its primary bearings from what he calls the competing natural law and republican traditions. In the author's view, the power of Rousseau's conception of political order owes a great deal to its novel integration of these two traditions, the former of which stresses the calculating individual, while the latter emphasizes patriotic virtue and education for civic freedom (pp. 211–12).

With an eye to this ultimate claim, Viroli examines Rousseau's varying conceptions of order as they relate to nature and society (or the "artificial") respectively. In the author's view, previous scholars have failed to place sufficient emphasis on these differences and have therefore mistakenly interpreted political order in Rousseau as a kind of return to nature. From the author's point of view, Kant is perhaps the most reliable guide to the meaning of political order for Rousseau, less for the former's universalization of that order than for his recognition of the artificial or ideal character of the social contract. Viroli argues that to the extent that the well-ordered society presupposes social interdependence it cannot take nature, in which men are radically independent, as its model. Instead, Rousseau looks to classical republicanism (in both its original and Machiavellian forms) for a remedy for social disorder—without, however, entirely abandoning the individualistic premises of modern natural law theory. The result, according to the author, is a constitution that is individualistic in its principles of justification, communitarian in its principles of preservation (p. 13), a combination whose inherent instability Rousseau himself was the first to recognize, as evident from, among other things, his conflicting and inconclusive treatment of international relations (p. 220).

Among the more useful consequences of the author's analysis is a sensitivity to the nonegalitarian strain in Rousseau's thought, and to his subordination (pace Marx) of "material" interests to vanity or amour propre as fundamental sources of human conflict. The author's analysis of the meaning of the social contract and resultant contrast between republicanism and despotism are especially helpful in drawing out the degree to which Rousseau falls short of being the advocate of participatory democracy for which he is frequently taken.

If there is a limitation in the author's approach, it lies in his failure to probe more deeply into the meaning of order generally for Rousseau, a question that might lead him to see more connection between natural and artificial order than he purports to see, and in a tendency to draw too simple and too absolute a distinction between the natural law and republican traditions than is warranted. (Here Melzer's *Natural Goodness of Man* [1990], an analysis of Rousseau's Machiavellinaism, is especially illuminating.) Moreover, Viroli's unquestioning attribution of the dualistic moralism of the "Savoyard Vicar" to Rousseau himself tends, in my opinion, both to confuse and to oversimplify the latter's approach to the problem of moral universals.

Reading Rousseau in the Nuclear Age begins, in some ways, where Viroli's work leaves off, discovering in Rousseau a coherent account of war and peace that others have missed. Roosevelt's point of departure is a novel and convincing reading of Rousseau's unpublished fragment on war and peace. Despite the somewhat misleading title of Roosevelt's book, hers is not primarily a study of the applications of Rousseau's ideas to nuclear strategy and other areas of immediate topical concern but a careful and judicious elaboration of Rousseau's understanding of the dilemmas and possibilities arising from the relations between states.

As the author sees it, Rousseau's own starting point is human innocence, or the contingency of human aggression on the (nonnatural) emergence of amour propre in human society.

The peculiarly dangerous character of international relations arises from the fact that while it resembles the natural state of man in some respects, it differs radically in others. "In contrast to the self-sufficient and naturally limited natural man, the state is an artificial being with no fixed or necessary limits to its size." Since, furthermore, a state's potentially limitless boundaries are necessarily "contiguous with those of its neighbors," opportunities for comparison and "capacities for aggressive expansionism are boundless" (p. 37).

Rousseau's remedies for this condition run, according to the author, in two directions: the first, a return to self-reliant (but nonaggressive) citizen virtue, as emphasized in such works as the *Social Contract* and *Considerations on the Government of Poland;* the second (less familiar to most readers) a moralization, through the sort of education suggested in *Emile,* of the relations between large, commercial states. It is in developing the second of these alternatives (along with its integration with the first) that Roosevelt's analysis is most original, raising the prospect of a more hopeful (though by no means optimistic)—and for immediate practical purposes, a more useful—Rousseau than the one suggested by Viroli. (One slightly negative note: the author's initial grounding of Rousseau's approach in a psychologically idiosyncratic need to believe in human innocence [p. 26] tends to undercut her later claims as to the pertinence of his analysis.)

Many readers will find the accompanying translations, including an emended version of *The State of War,* especially helpful.

SUSAN SHELL

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Scarcity and Modernity. By Nicholas Xenos. New York: Routledge, Chapman, & Hall, 1989. 122p. $39.95 cloth, $12.95 paper.

Nicholas Xenos, like a number of recent writers (Ignatieff, Hirsch, Baudrilliard, and Barthes), seeks to interpret the discrepancy between artificial scarcity and abundance—so central to Marx's critique of capitalist society. But unlike Marx, Xenos attributes artificial scarcity and excessive abundance to modernity in general rather than to the logic of capitalism in particular. The distinctive feature of scarcity and need in modernity, as Xenos understands it, is a dynamic that was pointed to by Adam Smith and Hume and has been given a recent articulation by René Girard, namely, emulative or mediated desire. We are driven constantly to experience scarcity in the midst of abundance because we are either seeking to appropriate goods and status items that are desirable simply because others want them or we are seeking to protect the desired items we do have from appropriation by others who would dilute their value. Consumption thus breeds a feeling of scarcity, which in turn breeds a need for higher more fashionable levels of consumption.

What sustains this dynamic? And how do we gain a critical interpretation of it? Xenos tries to take up this question from a number of different angles combining cultural critique, a critical account of the concept of "need" in social theory, and a brief history of the idea of scarcity and abundance. Xenos's reconstruction of the history of the concept of scarcity and abundance focuses on the peculiar fact that both the proponents of the competitive market (e.g., Smith, Hume, and the marginal utility theorists) and its critics (e.g., Rousseau, Marx, Mill at certain moments, and Keynes) all are indirectly caught in endorsing desire by emulation. On this reading, Adam Smith—against standard interpretations—endorses the social "illusion" (Marx's "fetishism of the commodity"?) that material wealth confers happiness and social esteem because this illusion has the useful by-product of stimulating the production of wealth (p. 16). For both Smith and Hume it is the desire for distinction through the display of wealth rather than the maximization of profit that is the driving force behind the market. From this initial statement of triangular desire at the root of classical political economy, Xenos traces the discovery and rediscovery of this principle through Rousseau's criticism of it and Marx's, Mill's, and Keynes's arguments for its eventual historical supersession to the marginal utility theorists' postulate of scarcity as a universal principle and the nineteenth- and the twentieth-century discovery of consumption as a system of signs of constantly shifting relational value.

This narrative serves a critical purpose as well. For embedded within it are a number of rather pointed criticisms of Rousseau, Marx, Mill, and Keynes—precisely the theorists who argued that the structure of emulative desire could be overcome. Although agreeing in part with their diagnosis of the structure of desire through exchange and comparison, Xenos singles out Marx and Rousseau for special criticism. Marx is guilty of invoking a "substantively empty" concept of authentic need in his *Early Writings* and then undermines even that in *Capital* with his notion that under communism needs will expand with productive forces (pp. 52–53). Marx therefore introduces unlimited desire by imitation into a society which has ostensibly overcome it. Now Xenos may have pointed to some serious problems in Marx's concept of abundance; but in making this argument, he conflates the process of discovering needs through control of the production process with the comparative stimulation of need in the market. Both are equated under the rubric of "unlimited need." A somewhat more differentiated argument is needed here.

Rousseau, by contrast, is criticized for seeking too stringent a means of abolishing mediated desire. The great legislator imposes virtue from the top while too much virtue is demanded from below. But this criticism is too briefly presented and more importantly never takes Rousseau's general will to be a serious theoretical answer to a very genuine dilemma posed by Smith and Hume's notions of socially mediated desire. For if all our gestures are interpreted as attempts to imitate what others desire, no motive, however worthy, is immune. Rousseau's only way out is to play the motivations bred by emulative desire against themselves so that individuals will use comparison as a motive to form a sovereign which will make such private comparison unnecessary. Participation in the general will indeed requires the sacrifice of emulative desire but this sacrifice is self-imposed, not coerced. Obviously one can dispute this point; but the important matter here is that it cannot be settled in the few brief pages with which Xenos treats the question. Curiously, Xenos himself ends the last chapter of the book with a plea against consumerism reminiscent of Rousseau: "Chasing an image of what we would like to be like, we are less likely to be satisfied with what we are at any moment" (p. 108).

But this raises the question what is the standard according to which Xenos finds false scarcity and consumption wanting? Having rejected the concept of "need" (because it is abstract and empty) and Roussean civic participation (because it is utopian and coercive), Xenos invokes the concept of the recognition of "limits" to expansive desire. In the discussion of Marx, for example, Xenos refers to Aristotle's concepts of natural limits to need (p. 52). Later, he invokes Weber's concept of "value rationality" as a potential standpoint against individual rational calculation (p. 78). But these are mere suggestions. The meaning of "limits" as a critical concept is left unspecified.

This signals a recurrent dilemma in this work, a dilemma that is strongly connected to the book's very strength, namely, its almost exclusive reliance on the theory of mediated desire as the interpretive principle for modernity. Although this principle illuminates and structures the debate over consumption and need, it also seems to serve as the answer to all critical questions; that is, Xenos deploys this theory without providing some account of its limits as an "interpretation" of modern desire. At moments, emulative desire appears to be a distinctive cultural construct of the West that appeared in the eighteenth century along with mercantile commerce and that continues to code our present behavior in consumer society. At other moments in this book it is portrayed as a feature of desire itself; and all theorists who sought to get beyond it (like Rousseau, Marx, Mill, and Keynes) found themselves in one way or another caught within it if only because they mistakenly regarded mediated desire as a necessary stage on the way to its supersession.

This said, however, I think this book definitely deserves to be read, and read carefully. Though rather brief, it is elegantly written, thoughtful in its juxtaposition of social and political theory with social history and culture criticism, and refreshingly modest in its claims. This last quality is especially appreciated, at least by me, when mediated desire in academia encourages inflation and hyperbole.

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AMERICAN POLITICS

Command, Control, and the Common Defense. By C. Kenneth Allard. New Haven: Yale University Press, 1990. 317p. $25.00.

The soldier–scholar is a rare phenomenon; perhaps this is to be expected, given the demands of either profession. But when one comes along, he brings an added dimension to his work: the directness, clarity, and focus of the former combined with the meticulousness, thoroughness, and care of the latter. When the work in question is also of immediate professional concern to the author, as in the case of *Command, Control, and the Common Defense*, there is reason to expect an authoritative and well-argued case with practical overtones. Kenneth Allard does not disappoint. Those with responsibility for the organization and direction of United States defense or who have professional concern for the proper and efficient conduct of national security, ignore this book at their cost.

The title of the book does little to disguise the author's intellectual debt. Samuel Huntington's *Common Defense* (1961) sets the theme of the book by focusing attention on the problems of central organization of defense and the growth of service autonomy. For historical, constitutional, and in some cases technological reasons, service autonomy in such matters as equipment procurement, operational planning, and the conduct of war not only grew but also became consolidated despite the steady trend toward combined planning and operational symbiosis during the latter part of the twentieth century. An expert in defense communications, Allard juxtaposes the necessity for central command-and-control over all United States forces in the planning, deployment, and use of military force against the traditional services' attitude that such centralization would be to the overall detriment of their effectiveness and efficiency.

The arguments have been articulated frequently enough, and been the subject of books such as Demetrios Caralay's *Politics of Military Unification* (1965) and, more recently, Robert Art and colleagues' *Reorganising America's Defense* (1985). But whereas these earlier books address the politics of central defense organization and control, Allard gets to the heart of the problem. He understands the implications of the technicalities and architectures of command-and-control in the modern battlefield and demonstrates that with today's information technology there is need for political–military leadership at the center to embrace it and ensure that it is used to the best operational effect.

Herein lies the major contribution of this work: Allard's detailed knowledge of modern tactical and strategic command-and-control of the U.S. Armed Forces and his analysis of their implications for both political control (as a civil–military relations issue) and operational effectiveness (as a professional military issue) advance the debate about the future control of the U.S. defense forces and central defense organization significantly.

Of concern to the U.S. taxpayer and the U.S. Armed Forces are the images of confusion and breakdown in service cooperation generated by such incidents as the invasion of Grenada. Whether true or not, the very credibility of these impressions suggests that something is very wrong in the U.S. military command structure from the president as commander in chief, down to the army, navy or air force private. Allard has sought to establish, through historical analysis, why this has come about; and in so doing has provided a carefully structured examination (chaps. 1-4) of the development of the command structures of the army and navy, with reference to technological change, war experience and their distinctive operational environments. Chapters 6 and 7 examine in detail modern developments in command-and-control and their impact on the conduct of modern warfare.

The two parts of the book are linked with a central chapter (chap. 5) that relates the question of service autonomy (with their own battlefield command and communication systems) with the wider question of defense centralization and joint command-and-control. As Allard observes, "The issue was this: the services had institutional expertise and procurement responsibility for command and control systems, . . . the Joint Chiefs of Staff had primary interest in joint command and control, . . . the unified and specified commands had all the responsibility for the nation's combatant forces, . . . and the Office of Secretary of Defense had . . . complete responsibility for anything that anyone did or failed to do" (p. 146).

The remainder of the book is designed to remove the confusion created by these cross-cutting responsibilities by taking modern command-and-control (information technology) developments and formulating practical policy recommendations in line with U.S. civil-military traditions. His objective is to construct a joint strategic paradigm in which the U.S. armed forces constitute a "harmonious association of powers in which one or other may predominate, but none must be in opposition" (p. 259). Allard is well aware that such an attempt would have to be made in the face of immense military and political resistance.

The Grenada experience must not be repeated; neither Congress nor the president must lose their political control over the purposes of war; and the U.S. armed services together must harness the multiplier effect of modern technology to achieve strategic and tactical objectives efficiently and effectively. Kenneth Allard has laid down the guiding principles, identified the problems, and focused attention on the options. From his professional perspective, modern warfare in the information age demands tight standardization; if the United States is to survive, past pluralistic traditions have to be put to one side (p. 263). As he notes, command-and-control technologies offer a solution: they enable massive military power to be brought to bear without massing forces. They also retain democratic control over the battlefield. But as political scientists will attest, bureaucracies, unlike armies, are formidable adversaries when it comes to sensible, reasoned policies; and as Allard himself has demonstrated, military bureaucracies are perhaps the most formidable of all.

MARTIN EDMONDS

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War on Waste: Can America Win Its Battle with Garbage? By Louis Blumberg and Robert Gottlieb. Washington: Island, 1989. 301p. $34.95 cloth, $19.95 paper.

Blumberg and Gottlieb assess the nation's deepening crisis with its garbage by focusing on the checkered history of waste-to-energy incineration. Indeed, the authors do a service in drawing compelling parallels between mass incineration and the path traveled by nuclear power: both were trumpeted insistently as more modern, efficient, cost-effective and cleaner than prevailing technologies; yet both have proven unexpectedly expensive, technically frustrating, addicted to direct and indirect government subsidies, and buffeted by acrid disputes over their public health and environmental risks. Both technologies also reside within rather oligopolistic markets, which went through a cycle of heady expansion during the energy crunch of the 1970s followed by virtual collapse in the late 1980s. All in all, apt parallels.

Fortunately, *War on Waste* goes even further. In their examination of mass incineration—and supported by a nicely illustrative case study of the demise of one particularly ambitious waste-to-energy project in Los Angeles—Blumberg and Gottlieb largely succeed in pondering the broader questions that grow out of this society's production, consumption, and disposal of what eventually becomes solid waste. It is, all told, a story of a society that once reused or recycled virtually everything but which today cannot somehow figure out its way from beneath an ever-staggering burden of trash.

Contradictions and vested interests abound, as one might expect. Recycling—that seemingly unadulterated good—is key to any worthwhile waste reduction plan; yet those who own or run waste-to-energy incinerators need plenty of trash to keep their plants running profitably. Plastics, damned as the embodiment of the throw-away culture, are not totally bad: they burn well, thus improving incineration's efficiency. Incineration *reduces* but does not eliminate waste: the remaining ash contains residues so toxic that many argue for their disposal in hazardous waste facilities—but that's another story.

Hovering above all of this is that fundamental struggle between individual freedom and the common good. Blumberg and Gottlieb tackle this last dilemma when they conclude that only a comprehensive and citizen-driven strategy of waste reduction will pull this nation out of its trash crisis. Incineration is not the panacea its supporters advertise, just as the sanitary landfill went from being the model of scientific management to an acute problem in itself in a few short decades. Recycling alone cannot solve the crisis, although it certainly can reduce the pile of trash far more dramatically than industry interests might admit. Only when the nation questions its fundamental patterns of production and consumption can any real progress be made—a remedy that is at once evident and painfully problematic.

War on Waste is an informative addition to the library of any student of environmental policy and politics. The authors frankly do not add much to our broader understanding of how public policy is made—nor, it seems, is that their purpose. The reader will find the work's bias rather obvious, but that should not deter anyone interested in knowing more about what has become a pretty complicated policy realm. It is concise, nicely analytical, and reasonably well written. *War on Waste* would prove most useful in upper-level undergraduate and graduate courses on environmental policy and public policy generally, although there are facets (e.g., the case study of Los Angeles) that might prove very instructive in courses on U.S. politics or business-government relations.

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No Safe Place: Toxic Waste, Leukemia, and Community Action. By Phil Brown and Edwin J. Mikkelsen. Berkeley: University of California Press, 1990. 200p. $24.95 cloth.

Site Unseen: The Politics of Siting a Nuclear Waste Repository. By Gerald Jacob. Pittsburgh: University of Pittsburgh Press, 1990. 224p. $29.95 cloth.

Over the past 15 years, numerous books dealing with the politics of hazardous materials or science and technology have examined similar themes such as the role of the public in shaping the form or decisions exercised by professionals in the public or private sectors and the tension that arises from efforts to reconcile these goals. Put simply, the age-old conflict between expertise and democratic administration that is often played out within the pages of organizational theory texts and cases is reprised here within a new format.

The works listed above address these concerns, albeit from differing policy areas, disciplinary perspectives and modes of analysis. Phil Brown (a medical sociologist) and Edwin Mikkelson (a psychiatrist) have written a case study of how community activists in Woburn, Massachusetts forced government officials to acknowledge and eventually respond to toxic contamination of local water sources despite formidable opposition from both public and private sector representatives. Gerald Jacob (a political geographer) focuses upon the use of contextual factors associated with the larger issue of nuclear power production to explain the eventual decision by the U.S. Department of Energy to site a high-level waste repository at Yucca Mountain, Nevada.

The question of "who decides" is a key concern discussed in both books. According to Brown and Mikkelson, concern over the failure of local or state administrators to address an inordinately high number of deaths from leukemia among children in Woburn led angry relatives and neighbors to develop an ad hoc organization titled For a Cleaner Environment (FACE). Public pressure, according to the authors, was necessitated by the unwillingness of corporations to recognize anything akin to socially responsible behavior and by inability of public health or environmental protection agencies to reconsider "scientifically correct" motions of epidemiology in favor of a model of science that better met the practical needs of the community. Members of FACE formed an alliance with health care researchers affiliated with Harvard University's School of Public Health to identify and document the proximity of neighborhoods with a high concentration of health problems to the areas thought to be heavily contaminated from toxic pollutants.

By training nonexpert but committed volunteers to conduct and process surveys, the authors contend that health care analysts were able drastically to cut the administrative costs associated with the production of a long and detailed community health study without compromising professional research standards. Accordingly, the use of "popular epidemiology" became a critically important means of establishing both the evidenciary base for legal actions taken against polluting firms and a catalyst of sorts to induce local administrators to undertake corrective actions.

The role of the public is less central to Jacob's examination of factors affecting the siting of a nuclear waste repository. He argues that not-in-my-election-year-like explanations commonly used to account for public opposition to hazardous waste decisions are overly simplistic because they neglect the importance of more fundamental structural variables. Who, then, is responsible for molding key decisions involving the disposal of nuclear waste? Jacob suggests that we consider the proposition that current policy actions are shaped by the same political actors that have dominated nuclear power policy questions since the early 1950s. He describes the nuclear establishment as a broad coalition of players that can be classified within a geographic dimension (i.e., national, regional/state or local) and a political economic dimension (i.e., governmental and economic interests plus organizations contained within civil society).

While Brown and Mikkelsen point to the political advantages that accompany the merger of community activists with professional groups in the pursuit of jointly held policy goals, Jacob concludes that victories or losses of this sort are of less importance in the long run than the ability of a policy coalition to restrict challenges to the content of existing program objectives, retain the basic structural distribution of political authority, prevent changes in decision processes, and maintain a sufficient resource base. He documents the political staying power of the nuclear establishment by analyzing the legislative battles associated with the Nuclear Waste Policy Act (NWPA) of 1982 and amendments to the act adopted in 1987 which pitted the Department of Energy (DOE) against environmental groups and organizations representing the states. By comparing the contested issues with the substantive policy outcomes, the author concludes that the coalition has successfully fended off challenges to its political authority. The NWPA alone attests to the successful strategy pursued by DOE and other members of the coalition to disentangle the waste disposal issue (which had emerged as an economic Achilles heel to the subsequent construction of new power plants) from the larger issue of where nuclear power fits into the U.S. energy picture within the foreseeable future.

Aside from the common elements such as concern about decisional authority and the hazardous nature of the policy problems under review, it should be evident by now that these books are quite different in other respects. In large part, each book is seeking a different audience, which in turn influences the approach adopted. Brown and Mikkelsen are reaching out to a broader set of readers with a piece of work that is evenly divided between case analysis and policy prescription. Their chief hope is to convince lay people of means of correcting serious policy biases ingrained within the fabric of society, as well as traditional notions of doing science. Despite a style of number of policy options that would remove some of the more formidable institutional hurdles to the pursuit of change by community action groups.

On the other hand, Jacob's analysis is clearly aimed at an academic audience and is oriented toward the refinement of policy theory. While adding a spatial component to analysis of policy issues is not terribly new (see, e.g., Ackerman and Hassler's classic study on clear air and Getz and Walter's work on hazardous waste), its juxtaposition with the political economic dimension to examine policy construction (as well as expansion) is both theoretically provocative and compelling in its presentation of evidence. This book contributes nicely to the literature of both environmental politics and policy studies.

CHARLES E. DAVIS

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Actors, Athletes, and Astronauts: Political Amateurs in the United States Congress. By David T. Canon. Chicago: University of Chicago Press, 1990. 180p. $32.00 cloth, $14.95 paper.

Few books successfully challenge important tenets of prevailing wisdom, and even fewer prompt us to think more deeply about a subject whose basic features we have long accepted. By redefining what constitutes amateurism in elective politics in the United States, David T. Canon's *Actors, Athletes, and Astronauts* accomplishes both unlikely feats and does so with considerable flair.

If there has been one universally accepted formulation in the literature of congressional elections, it is that of the quality candidate and the strategic politician. To be successful as either a challenger or a claimant on an open seat seems to require previous public office. We accept these assertions not only because they have been so convincingly documented by Gary Jacobson and others but because they make intuitive sense. While Canon does not assert that Jacobson is wrong, he asks us to accept a new and more complex definition of what constitutes amateurism in U.S. politics. But he does considerably more than that: in the process of detailing those circumstances under which amateurs do, in fact, win seats in Congress, he also challenges—quite impressively in my estimation—the well-established position of Joseph A. Schlesinger and other ambition theorists that the professionals are invariably better positioned to win office in times of great political turmoil.

Canon's redefinition of political amateurism presents us with three useful components: hopeless amateurs, the kind of lost-cause candidates who occasionally pull off an upset; the policy amateurs who are typically protest candidates or ideologues; and ambitious amateurs who differ little in campaign behavior or subsequent legislative activities from those who have held previous public office.

The most visible political amateurs who make their way to Congress—John Glenn, Bill Bradley, Pat Moynihan, or even former radio commentator Jesse Helms—may fit into any of these categories. You can, accordingly, vault into Congress without ever having presented yourself previously to an electorate and become a mover and shaker like Phil Gramm or Bill Bradley, become an ideological presence like Jesse Helms or Bob Dornan, or fall flat on your face like war hero Jeremiah Denton or astronaut Jack Lousma. If this suggests that amateurs in politics may not be all that different from other kinds of candidates, then Canon has taught his lesson well in suggesting that the line between amateurs and professionals is not as hard-edged as we once thought.

Canon argues convincingly that amateurs are more apt to find themselves with certificates of election at the very time that the ambition theorists have been telling us are the most auspicious for ambitious professionals. Electoral upheavals such as 1974 or 1980 seem to infuse Congress with a supply of political newcomers.

Compelling as Canon's conclusions are for the House of Representatives, they apply with much less force in the Senate, as Canon himself concedes. For starters, the truly high-profile amateur may not deign to be just one of 435 House members; only the patina of the Senate can add to the lustre of an established reputation in sports or Hollywood. Senate amateurs, once elected, seem to fare better than those in the House. For one thing, they have six years in which to discover that a phenomenally successful career shooting fouls or selling software doesn't entitle you to step into Daniel Webster's shoes. The lengthy Senate term enables you to make mistakes that you may afterward repair. Well-known newcomers may also chafe at being relegated to the back benches in the House.

If I find any fault with Canon's work, it is that he is not as rigorous as he might be in his commendable redefinition of amateurism. I find it difficult to swallow the assertion that Howard H. Baker and Russell Long were Senate amateurs in the same sense that Gordon Humphrey or Chic Hecht were. For Russell Long politics was very much the family business and to say that he was a novice causes a reader to scoff at an otherwise meritorious effort to explicate political amateurism.

More irritating than serious is that many times throughout the book, its origins as a dissertation come jutting up through the smoothly edited skin of the text like a jagged compound fracture. The tendency of the author to proclaim a little too loudly the novelty or revolutionary quality of his findings sounds like someone trying to impress skeptics on a dissertation defense committee. Anyone familiar with the literature will know just what a good job David Canon has done and need no rhetorical red flags to pick out the innovations he offers.

Like all of us, in the full flush of confidence in a challenging theory or provocative finding, the author gets carried away with the revolutionary import of his work. In Canon's case, however, his enthusiasm has some genuine warrant. He gives a serious jolt to our comfortable ideas of electability and political opportunity. For a work that employs a variety of sophisticated methods and statistical operations, *Actors, Athletes, and Astronauts* is a highly readable book that makes some important points about contemporary U.S. politics. Not the least of these is that the precious gift of name recognition, once vouchsafed only to those who held public office, can be achieved with third billing on a television sitcom or peddling building supplies in intermissions of "The Late Show."

ROSS K. BAKER

Rutgers University

Challenging the Boundaries of Reform: Socialism in Burlington. By W. J. Conroy. Philadelphia: Temple University Press, 1990. 264p. $39.95 cloth.

W. J. Conroy presents a case study of politics in Burlington, Vermont during the tenure of independent socialist mayor, Bernie Sanders. By itself, the story of Sanders's success in achieving office in 1981 and retaining it throughout Ronald Reagan's presidency, has the elements of a fascinating political narrative. Add the national attention that followed Sanders's victory in the 1990 congressional elections—the first independent elected to the House of Representatives since 1972, the first socialist member in half a century—and the case study has the potential of attracting a broad readership.

Conroy's book, however, stems from his Ph.D. dissertation on the limits to radical urban reform, not from a desire to explain how Sanders managed so deftly to rise against an apparent conservative tide. Indeed, the major thesis here is that the federal system, responding to the needs of capital, almost always acts to thwart what Conroy, following André Gorz, calls *democratic structural reforms.* These reforms, which Conroy clearly favors, change a structure to benefit "those who are dominated by challenging the logic of that structure. Such a reform puts human needs and demands (which ideally should be defined by the citizenry itself) ahead of the needs of a dominating structure" (pp. 68–69). In contrast, liberal reforms, while benefiting the disadvantaged, "accept the logic of the structure under challenge" (p. 69). Worse yet are reactionary reforms "that actually increase the authority and command of resources to structures that dominate" (p. 69).

Alas, whenever Sanders and the Progressive Coalition, his allies on the Board of Aldermen, managed to pass even a liberal reform, their disgruntled opponents would appeal to the state legislature or state judiciary to strike it down. And most of that time, the state government would oblige, vitiating the reform with the logic of Dillon's Rule. As we might expect, Sanders survived through compromise, clever electoral campaigns, and perhaps the good fortune of Burlington's geography: the city gained an overflow share in the largesse of Michael Dukakis's short-lived Massachusetts miracle.

In any case, Conroy relates how proposed reforms were defeated or largely compromised in the areas of economic development, taxation, and "sexual politics." Even in the largely symbolic area of foreign policy—Burlington adopted sister cities in Nicaragua and the USSR, Sanders backed resolutions protesting the invasion of Grenada and a State Department regulation that in the name of national defense, forbade Soviets from visiting the Burlington area (Chittenden County)—Sanders sometimes compromised. In particular, he opposed the Burlington Peace Coalition's demonstration of civil disobedience against the local General Electric defense plant, which also happened to be the area's second largest employer.

Though Sanders is a central character throughout the narrative, the focus remains less on his political career than on impediments—local, national, and especially state—to democratic structural reform. Conroy's ultimate aim is to develop a post-Marxist strategy for successful leftist reform. As such, his writing occasionally betrays a young man's contempt for Sanders's political pragmatism: "Pro-growth values were almost as ubiquitous in Burlington as they have been in most of America. Sanders's support for the Alden Plan [for private development of the downtown lakefront] contradicts any romantic notion that Burlington's local reformers opposed growth or were defeated by the ruthless onslaught of capital. . . . By 1989, the local Greens were running their own mayoral candidate against all of Burlington's pro-growth [including the Progressive Coalition] political parties" (p. 204). At best, over the eight years of Sanders's tenure, "a few liberal reforms were passed, and most reactionary reforms were either thwarted or delayed" (p. 198).

Where he might have concluded with an explanation of why or how the Sanders administration managed to remain popular enough for the citizenry to elect another Progressive Coalition nominee as his successor, Conroy spends the last chapter of the book outlining his proposed strategy for successfully achieving democratic structural reform. Suffice it to say that the strategy, which involves massive immigration of out-of-state leftists, has so moved readers that in the author's own words, everyone "who has commented on my prescription has done so negatively." But Conroy is not discouraged. Like the carnival barker who cries, "Don't knock my game unless you've tried it!" he will stick by his prescription "until it is tried and has failed, or some solution is tried that succeeds" (p. 254).

I may lament Conroy's apparent failure to attend to some of the most interesting aspects of Sanders's political success, but it is unfair to criticize the book he did not write. If readers will forgive the author's occasional tendency to beg the question of the rightness of his post-Marxist leftism, they will find an informative, well-researched, scholarly study.

MICHAEL MARGOLIS

University of Cincinnati

Race and Class in Texas Politics. By Chandler Davidson. Princeton: Princeton University Press, 1990. 329p. $25.00.

Texas politics pricks our political imagination with images of slow-talking, slow-moving politicians in western garb, of a state where conservatism is religion, and of amateurish good old boys dictating policy for citizens who are pacified with slogans of "frontier-speak," barbecue, and individualism. Cultural clichés have shrouded the real power struggles in Texas for some time, offering up simplistic explanations for conservative dominance in the state's political arenas. Davidson's iconoclastic work dashes most of these stereotypes while capturing the driving agents and paradoxes of Texas politics.

Underpinning Davidson's work is the paradox, Why should a state with such scope and depth of resources languish in responding to social problems? His main argument is that this is the outcome of class-dominated politics where conservatives have used violence, election laws, and money to hold off their liberal, populist antagonists. Further, conservatives have buttressed their dominance by fanning racial fears in order to drive wedges among threatening populist coalitions.

Myths become casualties of this systematic analysis as Davidson examines the themes of race and class. For example, although conservatives may dominate state politics, their struggle suggests that the political landscape is diverse and conflictual. Also, inherent in this struggle for control of state politics is the conclusion that conservative elites form an influential upper class, not a ruling class. Davidson notes the lengths they have pursued to win, and not always successfully; witness the presidential campaigns of Roosevelt and Johnson.

Davidson develops his investigation as a test of V. O. Key's theory that as race became less salient, class differences would drive conservatives, particularly the wealthy, to the Republican party. Hence, southern politics would manifest a two-party system, with the Democratic party playing the pivotal role for empowering the "have-nots" and allowing liberal factions to capture state political offices. To test this prediction in Texas, Davidson asks, Does class more than race explain political behavior in Texas? Do liberal-conservative struggles dictate Texas politics? (p. 16).

Davidson marshals evidence from the Civil War to the present that the "cultural folk conservatism" is a myth (p. 32). His attack lays bare the notion that most Texans are inclined to be conservative. Indeed, Texas has a rich history of Knights of Labor, socialist parties and newspapers, populists and extensive struggles of poor farmers and urban workers against the "plantation class" (pp. 5, 124). More recently, these struggles occurred within the Democratic party, with liberals gaining control during FDR's and Johnson's White House bids. Thus, it seems that Texas politics has diversity; more important, for a substantial period, liberalism dominated the grass roots. Then, how is it that conservatives controlled the party and virtually all elective offices?

Here is one of Davidson's finer contributions to state politics. He demonstrates that wealthy conservatives used white primaries, poll taxes, and six-month advance voter registration and (more recently) off-year state and local elections and heavily subsidized conservative candidates to restructure political participation (p. 151). These tactics hurt turn-out and campaigns for more liberal candidates. Thus, conservative dominance does not reflect cultural mores; rather, it is a product of biased election laws and saturated funding of conservative candidates (pp. 44, 59, 138).

Liberal-conservative struggles do structure political behavior in Texas, as Key suggested. Yet his prediction of the rise of two-party competition is diminished by the lack of have-nots' mobilization toward the Democratic party. Moreover, as well-financed conservatives fuel racial fears, working-class Anglos have moved to the Republican party. With support of the "super-rich," the radical Right, and now portions of the working and middle classes, the Republican party has been able to ward off liberal assaults on their bastions of power. Consequently, for Davidson, it is race that preempts class explanations in unraveling Texas politics (p. 239).

But Davidson's approach is not solely a test of Key's theories of southern politics; it is an in-depth examination of the social groups that vie for political consequence. In addition, as he develops his arguments, readers are exposed to strategies and tactics for control of convention delegates and primary rules that make state politics exciting (p. 183).

Some questions may arise over his use of dated studies to base generalizations about the present. In addition, his analysis does not extend far enough to explain why liberal female Democrats have recently captured the gubernatorial office and mayorships in Houston and Dallas. Still, Davidson's work illustrates how class and race questions block social programs in a state so laden in wealth. Although his work ends without proposing any solution to removing race politics, Davidson has illustrated how a core of wealthy elites depoliticize class conflict as they substitute irrelevant issues of sectionalism, threats of blacks and Mexican Americans, and local patriotism. Nevertheless, class and race issues create the tensions that drive Texas politics and ultimately tell us whose interests are being served. This book is an excellent read for students of state and local government, party politics, and ideology.

GERRY RIPOSA

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Agency under Stress: The Social Security Administration in American Government. By Martha Derthick. Washington: Brookings Institution, 1990. 231p. $32.95 cloth, $12.95 paper.

Many recent studies show that failures of policy implementation are often the result of internal contradictions, vagueness, or substantive error in the policies themselves. Derthick extends this analysis from policy to institutions. Her argument is that democratic elections, the presidency, Congress, the courts, and federalism, by their very institutional character, cause the failure of public administration through subordinating questions of administrative feasibility to the quest for policy innovation. Presidents, energized by their electoral mandates, seek to institute transformations in policy and do so with a rationalizing bias and a remoteness from the practicalities of field implementation. Conflicts between president and Congress lead to unclear and inconsistent statutory direction to administrators. Congress itself—responding to its multiple constituency, electoral connection—keeps changing its signals. And although it fails to care enough about administration to do a good job, it often resorts to detailed commands to administrators for policy or political reasons while simultaneously seeking to minimize the costs of administration. It wants administration to do more and better for less, so that it constantly creates situations that demand agency flexibility while simultaneously limiting agency discretion. Agency leadership often does not know enough to identify potential problems accurately. Aware of them, agency heads incline to underplay them out of reluctance to question the administrative prowess of their own agencies publicly or appear to be deserting the president's policy team through mere bureaucratic nay-saying. Moreover, policy makers often really have no way of assessing administrative capacity accurately except by trial and error. Federalism adds to the upset by requiring a consensual relation between levels of administration achievable only at high costs in administrative complexity that must be downplayed in order to achieve consensus.

Even the institutional arrangements for oversight of administration push in the same direction. As government has grown, we have sought to increase the democratic responsibility of the bureaucracy through greater oversight. That very increase in oversight has decreased agency autonomy. More generally the growth of government makes the tasks of oversight more difficult and multiplies the cost of errors. The presidency, Congress, and courts each claim ultimate supervision over administrative agencies that are "in" the executive branch but owe their existence and operation to congressional authorizing and appropriations statutes and for that reason must act within the "law," of which courts are the authoritative interpreter. All these breaches in agency autonomy by policy-driven institutions further undermine the agency's ability to defend administrative capacity against policy overload. The very administrative control the president wields is used to bend the agencies to the president's policy initiatives no matter what the administrative cost. Courts can and do sometimes impose their policy preferences on the agencies in the guise of legal oversight. In such instances it is usually the agencies, not the courts, who will bear the administrative costs, so the judicial overseers are likely to be far more entranced with whatever they see as policy virtues than with the costs of vindicating them. Congressional oversight is far more a matter of "fire alarms" than "police patrols"; and oversight that concentrates on emergencies is unlikely to balance administrative concerns adequately against what constituents perceive as human crises. Moreover, the agency's peculiar position in the separation of powers means that Congress can blame it as part of the executive branch while the president can largely avoid blame precisely by *not* defending the agency. And the agency is too dependent on both Congress and president to blame either for foisting undoable tasks upon it.

Because our administrative failures stem from our fundamental institutional arrangements plus policy makers' lack of concern for the administrative consequences of policy choice multiplied by the large size of government, fundamental correctives are unattainable. The purpose of our institutions is, and ought to be, to promote responsive, responsible, and limited government—not administrative efficiency. We are not likely to make government smaller. Our attention to, and intelligence about, administrative capacity might, however, be improved by such measures as periodic general reports by the General Accounting Office on the agencies and by an analysis of administrative consequences, prepared by the Office of Management and Budget, the General Accounting Office, or the Congressional Budget Office, for each piece of major pending legislation. Congress and the president would pay some attention to these studies if they were produced.

The author's argument is developed through the examination of two notable administrative failures—SSI and disability review—by an agency previously famed for its success. This book confirms my past experience that anyone interested in politics and administration would be wise to read very carefully everything Martha Derthick writes.

MARTIN SHAPIRO

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Television Access and Political Power: The Networks, the Presidency, and the "Loyal Opposition." By Joe S. Foote. New York: Praeger, 1990. 212p. $42.95.

This book's author argues that access to network television by the party not in control of the White House has assumed a central place in the democratic process. Without such access, Foote contends, the U.S. public can be manipulated by presidents who can command network air time and thus obtain public support unimpeded by an accountability-generating competition of ideas.

The book provides an interesting history of presidential uses of the electronic media beginning with President Coolidge and radio. It finds that presidents have always enjoyed privileged access to the networks' airwaves, whereas the opposition party has had only fitful success in obtaining opportunities to reply to presidential speeches and media events. In the pretelevision period, much radio time could not find advertiser support. Combined with the networks' "public trustee" concept of their responsibilities, this led the networks to offer themselves as a public forum that included substantial access for opposition spokespersons. With the rise of the three major television networks from about 1950 through the 1970s, opposition access diminished. The networks made seemingly arbitrary decisions about granting the opposition access for replies, using constantly shifting criteria and rationales. Increasingly, the opportunity cost of air time weighed against giving free access for any purpose; and even presidents occasionally had to coax or bully the networks to get on the air.

Foote shows, somewhat counterintuitively, that the president who did the best at avoiding opposition replies was Jimmy Carter and that the president most likely to be followed by rebuttals was Ronald Reagan. Foote argues that in Reagan's case, the networks consciously decided there was a need to counterbalance such a master communicator. Foote believes the networks should not exercise such unregulated power over the quality and structure of the nation's political discourse and recommends granting the opposition guaranteed access for replies to the president.

The book graphically illustrates that the declining size of the big three networks' audience has reduced the number of citizens that a president can reach by commandeering the airwaves. The president's share of the prime-time audience declined from 78% in 1979 to 58% in 1986. But Foote argues that ABC, CBS, and NBC will still be the primary vehicle through which presidents reach large audiences. Even though economic pressures are militating against the networks' traditional commitment to coverage of such presidentially dominated events as political conventions, Foote observes that the networks need presidents as much as presidents need them. Says Foote: "Presidential coverage is one proof that the networks remain as a key link between the people and their government. Losing that special entree to the American people, including presidential broadcasting, would rob network broadcasting of its heart and soul" (pp. 166–67).

The study's findings are not placed in the theoretical context of the literature on presidential relations with the public, typified by Hart's *Sound of Leadership* (1987), Kernell's *Going Public* (1986), and Lowi's *Personal President* (1985). Nor does the book do much with the many other channels besides televised speeches through which the president and his opponents attempt to communicate to the public. Thus, while Carter may have avoided much direct Republican reply time to his speeches, it would be difficult to argue that opposition to him had much trouble entering the media or public consciousness. Nor does it seem that Reagan, though consistently counterbalanced by reply time, was much constrained by it. As Foote himself suggests when he criticizes the Democrats for failing to use their reply opportunities against Reagan effectively, merely having time to say *nay* to presidential speeches is not enough to create an effective public dialogue on an administration's policies and pronouncements.

Still, Foote's book is a useful addition to the literature on presidents and the media. The book's basic theme and data appear solid and its recommendation for guaranteed opposition access is difficult to challenge, unless one takes an absolutist perspective on broadcasters' First Amendment rights—a view that happens to be ascendant right now at the Federal Communications Commission.

ROBERT M. ENTMAN

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Rhetorical Studies of National Political Debates: 1960-1988. Edited by Robert V. Friedenberg. New York: Praeger, 1990. 236p. $42.95.

The Six O'Clock Presidency: A Theory of Presidential Press Relations in the Age of Television. By Fredric T. Smoller. New York: Praeger, 1990. 170p. $39.95.

Rhetorical Studies of National Political Debates consists of eight essays examining nationally televised interparty debates from 1960 to 1988 and one essay that provides a synthesis of the findings. *The Six O'Clock Presidency* offers an explanation for, and data about, the nature and tone of television network news coverage of four presidents during the period from 1969 to 1985.

The single common thread involving both of these volumes is their focus on televised political communication: presidential and vice presidential campaign debates in one instance; network news coverage of presidents in the other instance. This common focus on televised political communication suggests a weakness that both volumes share though it is more pronounced in the Smoller book, namely, largely textual analyses, stressing verbal as opposed to visual communication.

Yet television is a unique communication modality. Many scholars acknowledge that what a communicator says on television counts less than in traditional public address, print, or radio. This is because television relies more on visuals, requiring less active involvement in message processing by receivers, and producing a decided shift away from verbal content and toward visual images. Consequently, the factors that are responsible for influence are different for television than for alternative communication modalities.

The point is that televised political communication cannot be fully understood by scrutinizing communicators' verbal messages. The Friedenberg volume hints at this. Sauter's chapter about the 1976 Mondale and Dole vice presidential debate argues that television was partly responsible for Dole's ineffectiveness. In analyzing Dole's "ascerbic comments," Sauter posits that Dole's humor, which was usually effective in live appearances, failed him in the television debate because he delivered his remarks without a smile and because reaction shots of the audience were not shown on television (pp. 59-60). Ritter and Henry's chapter about the 1980 Carter and Reagan debate argues that Carter lost the debate on both substance and style, reasoning that Carter's attacks were ineffective in part because he overlooked "the visual dimension of a televised debate" (p. 82). They add that "Carter failed to coordinate his nonverbal behavior with his verbal messages" (p. 83). Finally, Decker's analysis of the 1988 Quayle and Bentsen vice presidential debate suggests that the candidates' nonverbals helped determine viewer impressions of both men (p. 179).

The Smoller book acknowledges that the visual dimension is an important component of television network news but proceeds to assemble a data set based exclusively on verbal content. As Smoller admits, "The programs were not viewed directly; instead, the verbatim microfiche transcripts prepared by CBS were read and coded" (p. 2).

Beyond this single common thread, the two books claim very different purposes and vary in their success in achieving them. The Friedenberg volume attempts to provide a micro understanding of each of the eight nationally televised interparty debates and to cull macro trends and patterns across the debates (p. xii). *Rhetorical Studies of National Political Debates* achieves both objectives.

Friedenberg asked the authors to organize their essays based on a common format, responding to questions about the decision to debate, the objectives of the candidates, the strategies employed to achieve these objectives, and effects. Friedenberg's efforts produced remarkable uniformity across the chapters, with the exception of the Ryan essay on the 1988 Bush and Dukakis debates. The uniformity in approach to the specific debates facilitates a macro view of debates, which Friedenberg undertakes in the final chapter. He concludes (1) that because of public expectations, debates have become a permanent fixture of presidential election campaigns (p. 190); (2) that more effective debaters are able to adapt to the visual demands of television communication (p. 196); and (3) that formats of debates, while less than optimal, are to be expected, given the dominant role of the candidates and the press—and the exclusion of the public—in predebate negotiations (p. 207).

Rhetorical Studies of National Political Debates, because it provides a uniform approach, integrating goals, strategies, and results, provides new insights about the 1960 Kennedy and Nixon debate (Windt), the 1976 Mondale and Dole debate (Sauter), the 1980 Carter and Reagan debate (Ritter & Henry), and the 1984 Bush and Ferraro debate (Trent). In addition, the volume provides macro insights about debates generally, such as the precedent-setting impact of the 1960 Kennedy and Nixon debates on subsequent formats (p. 1), the unique problems that women candidates confront in implementing strategies (p. 138), and the impact of debate format on communication outcomes (pp. 172–73).

The Six O'Clock Presidency attempts to provide "an empirical theory of television news coverage" and to support the position that television news coverage contributes to the decline and fall of contemporary presidents (p. xi). The book falls short on both counts.

Smoller's theory of television news coverage is delineated in chapters 2-4 and 6. He argues that network news decisions are made based on visual, entertainment, and melodramatic imperatives (pp. 28, 31, and 31-32, respectively). These arguments have been made previously. Smoller's theory also features the "four seasons of presidential news," which posits that television network news follows a rather predictable cycle in their coverage of presidents, beginning with a series of personal profiles, shifting to an initial examination of the president's legislative proposals, turning to premature policy evaluation, and concluding with presidential reassessment. The tone of network news coverage becomes increasingly critical as it moves into phases 3 and 4.

Smoller presents the results of the coding of a large sample of CBS newscasts from 1969 to 1985. He reports aggregate numbers and percentages, conducting no tests of statistical significance. The data indicate that CBS newscasts provided extensive coverage of presidents. During this period, 97% of CBS newscasts contained at least one story on the president, while the average newscast featured two to three stories (p. 44): "On average, 20 percent of a typical program is made up of presidential news" (p. 45).

The author also maintains that the tone of CBS coverage was negative, although it is unclear whether the data support this conclusion. Smoller's content analysis revealed that 60% of newscasts were neutral or balanced, 24% were negative, and 16% were positive (pp. 45, 47). Next, he analyzes the "net tone" of CBS newscasts year by year, concluding that "in only 4.5 of the 16 years studied . . . did the incumbent receive a net positive portrayal" (p. 49). However, it is doubtful that this conclusion would be sustained if subjected to statistical tests. In five of the years, for example, the difference in the positive and negative stories was less than five out of a total of one hundred stories (p. 48). In addition, Smoller acknowledges that CBS news coverage of presidents may be more aggressive than ABC or NBC, thus limiting the generalizability of these findings (p. 44).

Smoller attributes the increasing negative tone of the CBS newscasts to the decision by network executives to urge greater use of *moments* in policy evaluation stories. He characterizes a moment as a "visually and emotionally involving scene in a news story that appeals to a viewer's heart rather than his or her head" (p. 54). However, the author's coding scheme made no attempt to analyze moments. Furthermore, in the instances of more decisive negative tone, the context of national and world events seems to be a much more plausible explanation for such coverage (e.g., Watergate during Nixon's second term, the Iran hostage crisis in conjunction with hyperinflation during the latter phase of Carter's term, and a severe economic recession during the first part of Reagan's first term).

Smoller examines strategies that presidents have used to handle the press in chapters 7 and 8. He concludes that both Carter and Reagan were ineffective in combating negative press coverage, because Carter was too accessible to the press, and Reagan too inaccessible. Smoller examines possible solutions to the increasingly negative tone of network news coverage of presidents in chapter 9.

Both books are well written and should interest scholars with specialties in U.S. politics, political communication, or mass communication. *Rhetorical Studies of National Political Debates,* in particular, makes a valuable contribution to extant knowledge about televised political debates.

MICHAEL PFAU

Augustana College

Rationalizing Justice: The Political Economy of Federal District Courts. By Wolf Heydebrand and Carroll Seron. Albany: State University of New York Press, 1990. 308p. $24.95.

There are two important themes in this new work on the federal district courts. First, the authors examine changes in the work of the courts throughout the twentieth century and the effects of social, economic, and political environments on cases and outputs since 1950. They are especially concerned with changes from trials and due process to emphasis on administrative efficiency and negotiation. The environmental variables reflect previous political science research on the courts; but they also capture the message of mainline sociological theory, particularly the power of private corporate interests and the state. The authors generally find little influence of the corporate sector but substantial effects of the size and complexity of government.

The second and more normative theme is that the transformation of the federal district courts in the twentieth century has produced a crisis in the quality of justice. To Heydebrand and Seron, changes from a system in which judges ruled the courtroom and due process and trials were paramount to the current system in which judges share power with others in an integrated organization and are mainly concerned with judicial administration create a threat to the fundamental purpose of the courts and their "democratic promise" (p. 3). They conclude with a discussion of six models of justice as ways of evaluating and reforming the courts. Their democratic model of justice appears to be their ideal, in which people have easy access to justice and get their day in court and various controls limit the power of lawyers and the state to rationalize the system to maximize efficiency and routine.

Their work on both of these themes is stimulating and provocative; but the themes do not fully complement each other or form a convincing case that change has resulted in a crisis that needs to be resolved. The empirical work is impressive and valuable as the first comprehensive analysis of its kind of the federal district courts. It demonstrates that environmental conditions have affected the work of the courts differently over time and that added resources have been devoted to particular types of cases and stages of litigation. Occasionally, however, heavy data analysis is not paired adequately with theory and explanation, leaving unclear why certain variables have been included in particular regression equations and the meaning of the relationships. Nevertheless, this book will be essential reading for anyone conducting research on the federal courts.

Much more debatable is whether the growth of government and increasing rationalization of justice is a problem. The authors apparently believe that justice was better before 1940, when there were more trials and simpler court organizations, than at present, when justice is obtained through informal procedures that in the authors' view are largely the product of government emphasis on law and order and the drive toward businesslike efficiency. But many other features of law and the judicial process also have changed in this century, making it unlikely, or at least uncertain, that litigants and defendants were better off in times past. Access to lawyers and public defenders has improved representation for underdogs; and there is generally much greater sensitivity to the rights of defendants throughout the country. The data in chapter 5 also show that the greatest decline in trials is in bench trials, not jury trials, and that jury trials are used most heavily today in criminal cases. Racial, ethnic, and religious minorities have much better access to the courts today than in the early twentieth century, and liability has been liberalized so that people harmed by the products of modern industry and society have much greater chances of compensation.

Finally, many litigants do not want a day in court but prefer a system where negotiation and compromise allow both sides to win—and lose—something.

Complete analysis of the transformation of U.S. justice requires a much broader inquiry and additional data than is possible to include in this book, where the primary emphasis is on the changing work of the courts. This is not a shortcoming, and the authors deserve much credit for linking their thorough analysis to broader issues of justice. While their work does not settle this debate, it contributes significantly to it.

HENRY R. GLICK

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The Symbolic Presidency: How Presidents Portray Themselves. By Barbara Hinckley. New York: Routledge, 1990. 193p. $35.00.

In this short but weighty volume, Barbara Hinckley brings together the presidency literature and the symbolic politics literature. She regards presidential rhetoric as the means by which presidents contribute to the symbolism of the office and as an important aspect of presidential development. Her argument rests on an equation drawn between the president and the nation he symbolizes. Her data set consists of presidential addresses to Congress and the nation from Truman through Reagan. Her analysis, which is rigorous and direct, reveals striking patterns across presidents.

Hinckley begins with the conventional view that the Constitution provided little definition of the presidency, thus leaving it open to development through the actions of presidents in response to the wishes of the people. The public activity of presidents, as manifested in *The Public Papers of the Presidents of the United States,* embodies the symbolic meaning of the presidency.

What emerges from her analysis of modern presidential rhetoric is an "institutionalized portrayal of the office carried on by the presidents themselves" that persists across presidencies and "takes on a life of its own" (p. 134). In that symbolic portrait "the president is the single head of the government and moral leader of the nation who speaks for and may be taken as equivalent to all the people. He expresses thoughts and feelings to the nation and does not engage in politics, an unnecessary activity in any case, since there are no major competitive actors or controversy among interest groups" (p. 134). In the symbolic presidency, the separation of powers is inconsequential, since Congress is only mentioned as a trivial participant in governance.

Hinckley is troubled by the constitutional and political implications of the symbolic presidency. The portrayal of Congress (in presidential addresses) as engaged primarily in taking vacations and running for reelection challenges the validity and utility of the separation of powers, while casting presidential policies in moral and religious terms undermines the separation of church and state. Moreover, unrealistic public expectations engendered by exaggerated presidential claims "invite disillusionment and do damage to the office itself" (p. 139). But Hinckley concedes that symbolic presentation is a source of power for the office and that harm could come from making the symbolism explicit. The challenge confronting presidents and the polity is to strike an appropriate balance between symbol and substance.

However, reform of the symbolic presidency will not be an easy task. Unrealistic perceptions of the office may lead to reforms that are not needed, for example, more power or improved staffing. Indeed, "in the real political world where controversy, complex events, and unreconcilable points of view permit no ultimate solutions, the office may be functioning as well as possible" (p. 144). Presidents are not all-purpose miracle workers and no set of reforms can make them so. If the people want a presidency that emphasizes substance over appearance, perceptions of the office must change. But presidents are major creators of false and misleading perceptions because "the job they are expected to do is at odds with the job they actually can do" (p. 145). They have presented a symbolic portrait of the office that is so largely at odds with reality that disillusionment and frustration are the inevitable result.

Ultimately, the solution, if there is to be one, must come from the people. The American people, Hinckley concludes, "need to think more carefully about the kind of presidency they want, since they will probably get the one they ask for" (p. 148). It is not at all apparent, however, that the public and many of the journalistic commentators want to change the office or that presidents are in a position to do so. The public is eager to idolize a newly elected president; and the more he tells it what it wants and expects to hear, the stronger the symbolic presidency becomes. The media depend heavily on a highly visible presidency and are often sharply critical of presidents who, like Carter, attempt to scale down the office or, like Bush, lack "vision" and maintain a relatively low profile. Academics, who in an earlier era contributed to the excesses of presidential symbolism through an exaggerated "textbook presidency," have recently displayed substantially greater objectivity and balance in their analyses of the office. However, their influence on the public and the politicians is questionable. Even careful and provocative arguments like Hinckley's are unlikely to persuade presidents to abandon rhetorical practices that have long-run negative consequences for the polity but produce short-run political gains for them.

NORMAN C. THOMAS

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Working within the System. By Ralph K. Huitt. Berkeley: Institute of Governmental Studies, 1990. 330p. $9.95 paper.

Nelson Polsby's Institute of Governmental Studies has done readers a favor by reprinting Ralph Huitt's work on Congress in one volume. It is an essential reference for students of U.S. national politics. After rereading many of his essays, I now appreciate the many ways that Huitt contributed to the behavioral study of politics.

Huitt was primarily a student of the Senate. He served as a legislative assistant to Senators Johnson and Proxmire and wrote pieces on Senate committees, norms, and leadership that remain required reading for all students of Congress. What will surprise less attentive readers of the professional literature on Congress is the scope of Huitt's work. For example, his perceptive essays on interbranch relations and legislative power in fiscal and monetary matters will be unfamiliar to many readers. They deserve a careful reading.

Huitt's scholarship was fundamentally, though not dogmatically, sociological. For Huitt, Congress was a distinct subculture: "Each house of Congress is a human group, with leadership, a hierarchy of influence, and a set of norms which control, more or less, the behavior of its members. The man who goes to Congress joins a going concern. He accepts and shares a group life" (*American Political Science Review* 48 [1954]: 340). Quite naturally, Huitt's explanation of congressional behavior generally took the form of detailing the hierarchy of influence and normative structure that members inherit upon assuming membership in the group. But Huitt clearly treated Congress as a *sub*culture that was quite open to the pressures and demands of the larger political culture in which it was embedded. Members' roles are defined by relations with outsiders as much as with insiders. For Huitt, republicanism, pluralism, bicameralism, federalism, and the separation of powers determine who those significant outsiders are. In fact, this volume is the place to begin for an introduction to the effects of these factors on congressional behavior.

Most of Huitt's essays are treatments of the inner workings of Congress. He emphasizes the endless variety found in Congress, which is what seems to have inspired him to study it. But at the same time he cuts through to the basic patterns. Each essay is elegant, rich, and sophisticated, bringing congressional politics to life while at the same time providing sharp analysis. His subjects are important, the difficulty of drawing inferences is always made explicit, and his conclusions are properly qualified. And he usually had more suggestions for further research than an army of political scientists could possibly pursue.

Huitt offered evidence in support of many propositions that we now take for granted. Consider just a few of them, all found in the pages of this volume: Committee members do not sit as judges of the public interest; rather, they serve as advocates of special interests. With the important exception of organizing the Senate, the claims of friendship or ideological allegiance take precedence over party affiliation for cross-pressured members. The Senate, probably more than the House, is quite tolerant of individualistic behavior. Senators fulfilling "outsider" roles help the Senate adapt to a changing environment. The successful Senate leader is one who helps colleagues effectively play their roles and structures roles so that a maximum number of senators can support the proffered legislation. Personal attributes have a great effect on the distribution of power in the Senate, which makes reliable assessments of the distribution of power in the Senate difficult.

Huitt's love of congressional politics is unmistakable in these essays. He did not think that Congress was without problems, of course. He favored reforms that would help the institution set policy priorities more effectively. But he was quite skeptical of more radical constitutional reform. He argued, correctly, that "there is no proof that hurrying legislation makes it better nor that anyone knows what legislation will solve problems." Huitt did not favor reform that would materially affect Congress's strength as a representative institution.

Seldom is it so convenient to take measure of a scholar's contributions to his field. Huitt, and this volume, will stand the test of time.

STEVEN S. SMITH

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The New American Political System. 2d edition. Edited by Anthony King. Lanham, MD: American Enterprise Institute Press, 1990. 364p. $36.50 cloth, $14.95 paper.

The original edition of *The New American Political System*, published in 1978, was a remarkable collection of essays that met a growing demand for a distillation and analysis of changes in U.S. politics since 1960. It portrayed the U.S. polity as increasingly fragmented and atomized, so that in the evocative words of Anthony King, "building coalitions in the United States today is like building coalitions in the sand." Essays by Samuel Beer on the search for a new public philosophy, Hugh Heclo on issue networks, and Martin Shapiro on the emergence of the Supreme Court as a major domestic policy maker set the intellectual agenda for a good deal of discussion and research during the subsequent decade. Thousands of graduate and undergraduate students were enlightened and provoked by what quickly became the standard text in the field.

Now over a decade later Anthony King returns to reexamine the "new American political system" with a new set of authors and essays. (Apart from the editor, only two of those who contributed to the first edition, Austin Ranney and Martin Shapiro, have also contributed to this one.) By and large, the individual chapters maintain the high standards of clarity and quality that characterized the first edition, but the volume as a whole is unable to achieve the freshness and impact of its predecessor.

The new edition consists of essays by Charles O. Jones on the presidency, Nelson W. Polsby on the Congress, Martin Shapiro on the Supreme Court, James W. Ceaser on political parties, Martin P. Wattenberg on the electorate, Austin Ranney on the mass media, Robert H. Salisbury on interest groups, Richard P. Nathan on federalism, Aaron Wildavsky on political culture, and King on the U.S. polity in the 1990s. Among the most provocative and informative essays are those by Jones, Ceaser, and Salisbury.

Jones argues that the U.S. presidency must be understood in the context of a government that has been transformed from a system of (in Neustadt's words) "separated institutions sharing power" (p. 1) into one of separated institutions competing for shared power. Standards for assessing strategies presidents use to achieve support and legitimacy and for evaluating presidential performance must reflect the "special challenge facing a president separated from, yet expected to lead, the rest of the political system" (p. 27).

Ceaser sheds new light on the old debate about the decline of political parties by distinguishing between party as an association of citizens who share a set of basic political views that they seek to advance by presenting candidates for elective offices and party as a set of more enduring institutional arrangements. Understood in terms of their dual nature, contemporary political parties are most accurately portrayed as at the same time and in different ways declining, stabilizing, and resurging. Rejecting deterministic explanations of party atrophy, Ceaser argues that party development is amenable to influence by political leaders.

Pondering the apparent paradox of the Tax Reform Act of 1986—armies of lobbyists wielding relatively little influence over policy results—Salisbury concludes that the growth in the number, variety, and sophistication of interest groups represented in Washington is more a sign of the weakness of special interests than of their strength. He contends "[that] a great many interest group representatives seek information more than influence, that in many ways they have become dependent on and are sometimes exploited by government officials rather than the other way around, and that much of what contemporary lobbyists do is to be understood as a search for order and a measure of predictability in a policy-making world that has been fundamentally destabilized by developments of the past twenty years" (p. 204).

In the concluding chapter, King finds that the picture of the new U.S. political system painted in the first version of the book remains largely true to life. An atomized politics continues to produce policy stalemate punctuated by bursts of frenetic activity. The latter—nonincremental lurches forward in public policy—often results from a change in the climate of opinion or a collusion of elites.

Yet King's tone is more respectful than critical. He is impressed more by the stability of U.S. political institutions than their change, more by the adaptiveness and resourcefulness of U.S. governing arrangements than their inadequacies. In stark contrast to the disaffection with U.S. government in this nation's political community, King cheerfully concludes, "As the old saying goes, If it ain't broke, don't fix it. And the American political system, even the new American political system, ain't broke" (p. 305).

THOMAS E. MANN

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Leading Congress: New Styles, New Strategies. Edited by John J. Kornacki. Washington: Dirksen Congressional Center and Congressional Quarterly Press, 1990. 185p. $35.00.

Recent events suggest that leaders may need more than "new styles and new strategies" to be effective in Congress. Scandals undermined the careers of three Democratic leaders in the House and Senate (Wright, Coelho, and Cranston). The Republican leadership was divided on the crucial budget compromise, with the House minority whip leading the opposition against his party's president. Most recently, the Democratic leadership in Congress meekly asserted its war-making powers while admitting that it could not enforce a party line.

A collection of essays edited by John Kornacki shows that the image of a chaotic, leaderless Congress is incorrect. If leaders attend to members' needs, exploit the power of the media, and use a variety of policy strategies, they still may be a potent force in Congress. The first half of the book is comprised of six brief essays that describe the new styles, strategies, and context of congressional leadership. The second half is an excellent essay by Barbara Sinclair evaluating research on the topic and suggesting directions for future work.

Each article contributes to our understanding of congressional leadership. Norman Ornstein points out that institutional context is critical in determining leadership style and effectiveness: Lyndon Johnson's style worked in the Senate of the 1950s, but it could not be used today. Ross Baker emphasizes the effectiveness of inclusive leadership and notes the importance of flexibility. Cokie Roberts provides the most entertaining essay ("What has two legs and is attracted to light? Phil Gramm"), while adding many new insights. For example, she notes that some members circumvent the leadership by taking their cases directly to the people, much like Samuel Kernell's observation that presidents often "go public." She also observes that satellite transmissions have led to the proliferation of local news people in Congress: the size of the congressional press corps increased from 855 in 1980 to 2,200 in 1988. Susan Webb Hammond and Steven Smith add an important dimension by noting the role played by members "without portfolio." Smith's essay is especially ambitious. His theory produces testable propositions and illuminates differences between the House and the Senate.

Evaluating this book is very difficult because its goals are so broad. The stated purpose of the Dirksen Center study that produced these essays was to examine the "origin, nature, and operation" of congressional leadership (p. vii). With such a sweeping mandate, not surprisingly, the authors pursued a variety of approaches: some were content to describe one aspect of congressional leadership, while others had more ambitious theoretical aims. Some wrote impressionistic accounts, others were more systematic. Some presented a map for future research, others showed us where we have been. All of these are worthy aims and techniques; but a single, slim volume needs more focus.

Ultimately, the book outlines many provoking questions; but it does not provide many answers. The culprit is the repetition, anecdotal evidence, and inconsistently specified concepts that are produced by the absence of an organizing framework. To cite one example, each essay has a different conception of leadership. Baker presents a pragmatic definition, based on realistic policy expectations; Hammond and Smith expand the definition to include anyone who has an impact on policy; and Ornstein argues that leadership varies with the changing institutional context.

Perhaps the most frustrating point is that Sinclair's essay discussing the external context and institutional environment, and emphasizing the expectations of party members, *could have* provided the framework if the Dirksen Center had commissioned it first. Each scholar could then have tackled a part of the leadership puzzle outlined in her framework. The end result would have been a more comprehensive contribution.

Instead, despite sharing the same topic, the two halves of the book are virtually unrelated. For example, Sinclair devotes two of her 65 pages to "informal leaders" in Congress and a page to the role of the media, yet three of the first six chapters are on these topics. Considering the sequence in which the papers were written, a greater proportion of the review essay should have been devoted to synthesizing and integrating the previous essays.

Given the most important contribution of this book, which is stimulating research on congressional leadership, I will conclude with a few thoughts about the direction of future work. Smith and Hammond are convincing that "informal leaders" play an integral role in the modern Congress. However, we need to know the extent of overlap between policy leaders, informal leaders, and formal leaders. To what extent are the various career paths within Congress mutually exclusive? When and how do the interests of these three groups intersect?

More central is how to theorize about something as apparently idiosyncratic as leadership in Congress. Clearly, individual personalities and styles are important; but they are not determinative. The concepts employed by Sinclair and Smith lead to two research questions that are most likely to produce generalizations. First, to what extent are leaders constrained by existing institutions? Or can they freely shape them? Leadership institutions that persist after leadership turnover indicate practices that are independent of individual leaders. Examples are the patterns of negotiating complex unanimous consent agreements in the Senate and using closed or complex rules in the House. Second, as suggested by Sinclair, what roles do leaders play on different types of issues? The normative interest in studying the leadership is that it provides a greater understanding of the policy-making process. A systematic analysis of policy leadership, both inside and outside the formal leadership, is an important next step. Steven Smith's project on this topic promises to be a path-breaking contribution.

Despite the absence of an organizing framework, *Leading Congress* is an essential starting point for future research on leadership and an important book for anyone who wishes to understand the modern Congress.

DAVID T. CANON

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Book Reviews: American Politics

Symbols, the News Magazines, and Martin Luther King. By Richard Lentz. Baton Rouge: Louisiana State University Press, 1990. 376p. $29.95.

Those interested in a detailed recounting of Martin Luther King's leadership in the Civil Rights movement, the clashes of ambition, will, and vision among the leaders of the movement, and the flow of events from the Montgomery bus boycott in 1955–56 to King's assassination and the Poor People's Campaign in 1968 will find utility in this study. It provides a chronological narrative and many anecdotes. But the book offers little beyond this and, indeed, does not fulfill the stated objectives of the author.

Richard Lentz, who teaches journalism at Arizona State University, states in the preface that this is "a study of King as a symbol and how that symbol was presented by the news media for more than a decade" (p. ix). This is to be accomplished by examining the treatment of King in the three major national weekly news magazines: the centrist *Time*, the slightly left-of-center *Newsweek*, and the right-of-center *U.S. News and World Report*. The title of the work signals an intent to focus on King portrayed as a symbol in these news magazines. It is not, however, a systematic examination of these weeklies' transformation of King into the symbol of the Civil Rights movement and the American creed in its black American evocation.

What Lentz offers is a recapping of the major events of King's leadership of the Civil Rights movement in this period and an evaluation of how it was reported and interpreted in the news weeklies. The reader learns how *Time* and *Newsweek* idealized King as an advocate of nonviolence, a reformer of the desegregated South, and a voice for the extension of the American Dream to black Americans; dethroned him when he ventured into more confrontational protest against racism in the North, poverty in the United States, and the Vietnam War; and made him an icon in death. The news magazines used Dr. King as a moderate and pacific counterexample to the more radical black leaders, such as Malcolm X and Stokely Carmichael (*U.S. News and World Report* to a lesser extent, for it was consistently hostile to King). One learns interesting tidbits to be stored for later use. *Newsweek* and *U.S. News and World Report* did not report or quote King's now famous coda to the Washington "I Have A Dream" speech: "Free at last, free at last, thank God Almighty, we are free at last." And *Time* did not quote it until four months after it was delivered. When the news magazines were unhappy with King and his activities, they omitted mention of his 1964 Nobel Peace Prize; indeed, *Time* also forgot that it named him "Man of the Year" in 1964.

What the reader does not learn much about is the author's purported "interest in cultural values and symbol making" (p. ix). The study reveals no apparent research strategy or design except to glean from secondary sources and the news magazines a chronological recounting of King's efforts in the Civil Rights movement and how they were reported and characterized in the weeklies. The author did not conduct any interviews with civil rights figures or news magazine journalists of the time. The methods employed are those of a journalist—anecdote-, event-, and quotation-based (ideographic) and focused on personalities rather than larger issues, movements, and trends. It offers a subjective and personal interpretation of the causes and meanings of how the publishers, editors, and staffs of the news magazines presented King and his leadership of the Civil Rights movement in the late 1950s and the 1960s. While one may agree with, and find satisfaction in, many of the author's interpretations, one is at a loss to find in the book the empirical foundations of the author's conclusions.

Lentz does not ground his research in any theory. The concept of "symbol" is never defined; and there is no discernible theoretical underpinning to the study, whether drawn from the social and behavioral sciences or literary analysis. Absent is any reference to the available literature in such relevant areas as symbols and cultural attitudes, symbolic politics, political communication, and unconventional social movements. Do not anticipate any use of content analysis in the work. There are no content analytic techniques employed, whether quantitative or qualitative, to examine the descriptors used in the news magazines to portray King, his words and activities, or the other people and events in the Civil Rights movement.

Lentz employs the conventions, norms, and techniques of journalism to tell an interesting and important story. But his study does not have the systematic framework, grasp, reach, and willingness to generalize that are appropriate to the subject and to the author's stated purpose. The task of gleaning from the study any general insights, principles, hypotheses, or theoretical speculation is left entirely to the reader.

The work of journalists and scholars of journalism is important to building a body of expertise and knowledge in the burgeoning and interdisciplinary field of political communication. It is important that the news media be recognized and studied as political institutions, and it is essential that political scientists be major players in that recognition and study. The field should not be left largely to journalism and communication studies because the perspectives and kit of tools—theories and methods—of political science are important to build understanding of political communication as process and the news media as institutions integral to politics.

HERBERT WALTZER

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The Modern Theory of Presidential Power: Alexander Hamilton and the Corwin Thesis. By Richard Loss. New York: Greenwood, 1990. 192p. $39.95.

How far have presidents and presidency scholars strayed from the wisdom of the founders regarding executive power? Quite far, argues Richard Loss, who calls for a return to the Hamiltonian-Washingtonian conception of presidential power.

Loss begins by arguing with Edward S. Corwin's thesis that "the modern theory of presidential power" is essentially a Hamiltonian notion. Loss argues that Hamilton did not advocate a presidency-centered view of the U.S. system. Instead, Hamilton calls for a republicanism based "on natural law, Christianity, honesty, justice, liberality, moderation and virtue" (p. 153). Hamilton's *real* message is one of "moderation," not the aggrandizement of presidential power. Presidents and scholars have misrepresented Hamilton's views and identify him with the strong-presidency model. Hamilton's views—and Washington's—reflect a more limited and republican (in both its classical and modern forms) approach to presidential power.

To argue this position Loss draws a distinction between the *Federalist Papers*, which were written as political propaganda and "of necessity lack complete candor" (p. 24) and thus do not reflect Hamilton's true beliefs and Hamilton's *Pacificus Letters*, which were "a corrective of the *Federalist's* concession to the fears of the state ratifying conventions" (p. 25). The *real* Hamilton is found in Pacificus. Loss's assertion is plausible but not convincingly presented.

If Hamilton and Washington are the good guys of this tale, the bad guys are clearly the "revisionist" presidents and scholars who misunderstand Hamilton's message and promote the aggrandizement of presidential power. Chief among the presidential culprits are Lincoln, "a revolutionary figure" who "abandoned Hamilton's crucial limitations that presidential power must be compatible with other parts of the Constitution and with the principles of free government" (p. 90); Theodore Roosevelt, who "implicitly denied Hamilton's moderate doctrine of restraints of power" (p. 93) and developed a "post-Hamiltonian" (p. 95) stewardship theory of presidential power; Woodrow Wilson, whose "theory of presidential power . . . squints at tyranny" (p. 103); and Franklin Roosevelt, who "went to a non-Hamiltonian extreme in threatening Congress" (p. 107). Surprisingly absent from this presidential hall of shame is a recent activist conservative, Richard Nixon.

Presidential scholars also populate the presidential rogues gallery, led by Edward S. Corwin, who incorrectly linked Hamilton to the presidency-centered model of U.S. government, Clinton Rossiter, who glorified presidential power, and Richard Neustadt, who "teaches a decayed, academic Hobbesianism" (p. 147).

Loss attempts to redeem Alexander Hamilton and rescue his historical reputation from successors who have misunderstood and misrepresented his view of presidential power. He challenges the accepted wisdom regarding Hamilton and asks us to reconsider our views of presidential power in favor of a more limited vision of power controlled by virtue and Hamiltonian moderation.

This thin volume is useful, if often too brief and unsatisfying. Presidency scholars would be well served by reflecting on Loss's challenge to conventional wisdom.

MICHAEL A. GENOVESE

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America's Misunderstood Welfare State: Persistent Myths, Enduring Realities. By Theodore R. Marmor, Jerry L. Mashaw, and Philip L. Harvey. New York: Basic Books, 1990. 268p. $22.95.

At least in the academy, liberals are preparing to retake the offensive in social policy debates. Smart responses to the conservative critique of the U.S. welfare state and well-crafted plans to make antipoverty policy simultaneously more generous and more attentive to the work ethic are proliferating. This book provides a useful, clearly written, and welcome précis of this new and improved liberalism. Marmor, Mashaw, and Harvey want to show that conservatives are wrong about many of the most important features of the U.S. welfare state—its impact on work, savings, and investment and public support for social reform. Along the way, they also seek to rebut social democratic and neo-Marxist criticisms that U.S. social policy is incoherent and internally contradictory.

According to the authors, U.S. social policy works: it is affordable and efficient. Contrary to accepted wisdom on the right, social insurance and public assistance programs reduce economic insecurity without imposing large deadweight losses on the economy or arbitrarily restricting individual liberties. Only the medical care system is out of control; but the problem here is too little—not too much—government.

The authors also think the U.S. welfare state is consistent and coherent. We have, they argue, an "opportunity-insurance state" (p. 22). By design, it is not residual or redistributional. And this is the welfare state that U.S. citizens want. The growth of social welfare is the result neither of a self-expanding welfare state bureaucracy nor rent-seeking special interests who selectively benefit from public provision. Nor is it an artifact of democratic voting procedures. Public opinion polls show, they argue, that contemporary social policy reflects a genuine societal consensus on mutual assistance within a framework of individual responsibility and limited government. If anything, organized opposition by conservative political interests and ideologues artificially denies citizens many of the things they want from the state.

Marmor, Mashaw, and Harvey are also hopeful about the future of reform. The continuing problems of economic insecurity—notably the poverty of children, access to affordable medical care, and social security financing—can be addressed incrementally, within the existing institutional framework. They propose more generous welfare benefits tied to work requirements and supplemented by job creation programs; a universal, national health insurance system; and modest reforms in social security taxes and benefits.

Much of this is compelling. The book works through the best available evidence on the impact of social policy carefully and effectively counters the most common conservative complaints. But there are some problems. First, the limits of existing protections are understated. In their brief for liberalism, Marmor, Mashaw, and Harvey sidestep the fact that the U.S. government does less to promote the economic security of its citizens than most other Western democracies; that it meets the welfare standard established by its European counterparts only on the most elementary welfare state commitments; and that it extends a rather limited safety net, implemented in ways that often discourage the poor and economically dependent and leave adult workers and their dependents at risk of sudden job and income loss.

Second, the account of the possibilities and limits of further reform is insufficiently attentive to the complex relationship among institutions, policies, and political beliefs. The authors' brief against redistributional reform is purely pragmatic: public opinion will not support it. But attitudes about social policy are shaped by what is. As the comparative literature on Western welfare states makes clear, the United States separates social insurance and public assistance—and the "deserving" from the "undeserving" poor—to unusual degrees. These institutions, in turn, reinforce longstanding racial antagonisms. It is likely that these arrangements condition what citizens think about social policy and that other, more universal, institutions might encourage other attitudes.

Finally, Marmor, Mashaw, and Harvey's resolute defense of incrementalism is not entirely convincing. As they themselves acknowledge, any work-oriented, antipoverty program will have to provide the poor with good jobs. But changing world and domestic economies are transforming the United States' industrial and occupational structures, proliferating low-wage and part-time service work, and threatening those who have traditionally relied on high-wage, blue-collar jobs. In this environment, it may not be possible to rely on incremental reform strategies. Government may have to take a giant step into the world of industrial policy, public enterprise, and active labor market policies if it wants to assure a ready supply of well-paid work. And to take this step, reformers may have to rethink the entire social policy enterprise and the role of government within it. To accomplish even small things, liberals may have to embrace large changes.

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Race, Class, and Education: The Politics of Second-Generation Discrimination. By Kenneth J. Meier, Joseph Stewart, Jr., and Robert E. England. Madison: University of Wisconsin Press, 1989. 194p. $37.50 cloth, $14.95 paper.

Politics, Markets, and America's Schools. By John E. Chubb and Terry M. Moe. Washington: Brookings Institution, 1990. 318p. $28.95 cloth, $10.95 paper.

Schools in the United States have been the subject of continuing public debate since the late 1950s. These two books examine what has happened in efforts to foster educational opportunity and quality, which have been central themes in the debates over the educational system.

Meier, Stewart, and England focus on equal educational opportunity while Chubb and Moe concentrate on the quality of education in the schools. Both books conclude that efforts to improve schools on these dimensions have failed. While the educational establishment is blamed for these failures to a great extent, the authors of the two books see significantly different paths to rectifying the situation. Specifically, Meier, Stewart, and England start from the premise that equal educational opportunity was the original objective of desegregation efforts but was lost in the battles over desegregation. Instead, second-generation discrimination developed in which black students were separated from white students through academic grouping and disciplinary processes. In disproportionate numbers, blacks are placed in lower-level academic groups theoretically based on ability and are more likely to be subject to discipline. For Chubb and Moe, the quality of education has suffered because of what they refer to as institutions of direct democratic control. Reform efforts have addressed policies but not the institutional aspects of education and thus have missed the mark in Chubb and Moe's analysis.

What ties these studies together is that the authors of both books attribute the problems to the fact that decisions about how to reform education or how to deal with desegregation are left to those with the greatest stake in protecting the system as it is. In desegregation, Meier, Stewart, and England note that educational professionals developed techniques that were considered nonracial in their implementation. However, the effects of these professional techniques sustained racial segregation by separating blacks and whites within the schools rather than by school. For Chubb and Moe, reforms over the years are viewed as efforts to depoliticize the process for decision making about education. But this leaves the education profession and others with a specific stake in educational policy in control of that policy, resulting in what the authors term overbureaucratization of the schools and education policy.

These studies examine data on school districts across the country. Meier, Stewart, and England utilize data from the Office of Civil Rights on large urban school districts enrolling 15 thousand students or more. Chubb and Moe build upon the High School and Beyond study (Coleman, Hoffer, and Kilgore, *High School Achievement*, 1982) in which 60 thousand students in one thousand high schools were surveyed. Chubb and Moe went back to about half of the schools and surveyed administrators and teachers.

Politics and political institutions are considered key to understanding public education for both sets of authors. What differentiates them, however, is that they have different views of the prospects for politics and political institutions to play positive roles in resolving the problems apparent in the U.S. educational system. Basing their analysis on the literature in urban politics dealing with the effects of minority representation on city councils and in administrative positions for policies responsive to minority communities, Meier, Stewart, and England examine the relationship between second-generation discrimination and black members on school boards, black administrators, and black teachers, among many other factors. While they find that black teachers are the most important factor in improving black prospects, they also find that all of these factors build upon one another in such a way that the proportion of black teachers is determined *after* deducting membership on school boards, black administrators, and the like.

Chubb and Moe examine student ability, family socioeconomic status, school socioeconomic status, and school organization. The first three variables appear to have small influences on student academic gains, but effectively organized schools can add half a year of academic gain over the last two years of high school. In particular, Chubb and Moe find that autonomy is the single most important element in the success of schools in academic gains. This finding is in sharp contrast to the view of Meier, Stewart, and England that intervention from the outside (federal government) is critical to local schools' improving the lot of black students. Although Meier, Stewart, and England do not focus on educational quality overall, their conclusions do indicate that all students benefit when equal educational opportunity is present. They also note that equal educational quality is subverted by second-generation discrimination unless the national government is aggressive in pressuring local officials into efforts to improve opportunity.

These two studies have differing interpretations of what is likely to be successful in improving education in the United States. Meier, Stewart, and England believe that politics matters and can be a positive force in improving education although most analyses tend to focus on how politics interferes with efforts to improve schools. Blacks should be encouraged by the fact that participation in the political process leads to changes that improve their opportunities for quality education. For the policy analyst, these authors also conclude that how policy is defined affects its evolution and impact. Thus, defining the problem of equality of educational opportunity in terms of desegregation of schools focused attention on desegregation battles and not on ensuring equal opportunity, resulting in resegregating within the schools with blacks relegated to lesser-quality internal programs and opportunities.

Chubb and Moe are pessimistic about the ability of politics to be a positive force. In fact, they are critical of the focus of social scientists on policy and recommend that attention be directed to institutional arrangements. Using private schools as a model, they suggest that autonomy of and within the schools is essential to improving them. Their prescription is for choice on the part of all concerned. While much attention has been given to parental and student choice in selecting schools in recent reform efforts, they would like to see the political system permit schools to have choices in how they operate and what they offer. By allowing maximum opportunity for the schools to function as they see fit (with some guidelines), they can be responsive to the needs for quality education.

Clearly, Meier, Stewart, and England's prescription for improvement has greater chance of being accepted in our society. Because of the constraints identified by both books, the chances of schools being granted the degree of autonomy suggested by Chubb and Moe is slight. Those who have control and a stake in maintaining control are not likely to give it up. In a democratic political system, it also is too much to ask to have the electorate give up direct democratic control over something as important as the schools. Those who wish to make a difference are more likely to be successful by participating in politics, as Meier, Stewart, and England suggest. The political scientist also is more likely to be comfortable with their analysis. Both books, however, offer good insight into an important policy problem and should stimulate new thought and debate on U.S. education policy.

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The Democratic Wish: Popular Participation and the Limits of American Government. By James A. Morone. New York: Basic Books, 1990. 402p. $22.95.

Like most eighteenth-century antiques, the U.S. political system has a certain baroque charm. It also has a ramshackle structure where a state should be. James Morone's explication of episodes in halting state building is an interesting account of the development of this peculiarly U.S. political condition. The product is political history with practical intent, for Morone builds morals for democrats on a foundation of missed opportunities and misdirected efforts.

To Morone, the major theme in U.S. political history is liberal individualist hostility to central power and a concomitant institutionalized incapacity to confront crisis. Morone's more original contribution comes in the form of the minor theme he identifies—the democratic wish of his title, invoked in crisis, never fulfilled, and always frustrated. The democratic wish is a communitarian, republican invocation of the people to move in place of incapacitated state authority. But the result is always disappointing. A republican revolution yields to a liberal constitution, Jacksonian populism produces patronage and spoils, progressivism degenerates into interest group liberalism, and the Civil Rights movement is bureaucratized via the War on Poverty.

The story is always the same: institutionalization compromises and corrupts the democratic impulse by grafting it onto the existing political structure, without really changing the rotten core. Persistent and pervasive fear of government means that reformers appeal to the consensus of an imaginary people and community yet dare not vest effective public authority in the state. But no matter how many times it comes to naught, the democratic wish lives on. Morone even finds it operating in less comprehensive conflicts, notably the reform of labor relations in the 1930s and the transformation of health care politics in the 1970s.

As political history, the argument is first-rate, though the more distant examples constitute clearer support for Morone's thesis than the more recent and more limited ones. Morone's contribution to our understanding of state building in the United States (especially its limits) is substantial and profound. Political scientists who hark back to Arthur Bentley on matters methodological might dismiss the democratic wish as "soul stuff," but no matter. The idea yields genuine insight, even if its empirical referents are sometimes a bit elusive.

Morone's own hopes for the future entail reinvigorated public authority achieved through the incorporation of the democratic wish in a strengthened state. This hope locates him solidly within what Raymond Seidelman and Edward Harpham, in *Disenchanted Realists* (1985), call the "third tradition" of U.S. political science. (The democratic wish in its naive form resonates with Seidelman and Harpham's "first tradition" of republican radicalism that begins with Tom Paine.) Morone's contribution suggests that this tradition has not expired in the custody of Theodore Lowi and Walter Dean Burnham, after all. However, Morone comes to the same impasse as Lowi; for in identifying the need for rejuvenated public authority, he also explains why that cannot happen: any such attempt is likely to be coopted by the very forces whose power and persistence Morone himself has chronicled.

If his preferred future is, as Morone himself concludes, "an unlikely aspiration" (p. 337), what hope remains? If the democratic wish can never be effectively conjoined with state authority, perhaps the answer lies in their more effective separation. And this is why some democratic theorists would now depart from Morone in looking for the fulfillment of the democratic wish not in any happy marriage with the political center but rather in public spheres explicitly divorced from and confronting the state.

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Presidential Accountability: New and Recurring Problems. By John Orman. New York: Greenwood, 1990. 171p. $39.95.

In this excellent study, John Orman has examined the capabilities of the U.S. political system to hold the presidency accountable, and his conclusions are distressing. Especially in the Reagan era, the imperial presidency has undergone a formidable revival and extension from what it was under Nixon and Watergate. In defying the usual constraints of accountability, Reagan pushed dangerously farther than Nixon, a deed that has been little recognized.

According to Orman, Reagan's secrecy and deception in "Irangate" were more offensive to democratic standards than Watergate. The sale of millions of dollars of weapons to Iran to secure the release of U.S. hostages violated the Export Administration Act of 1979, which restricts exports to terrorist-supporting nations. It contradicts Reagan's hard uncompromising stance against terrorism; and the administration's actions deceived Congress, the media, and the public. Funds that were diverted to the Nicaragua Contras from the arms sales to Iran circumvented the appropriations power of Congress—the historic power of the purse, which holds the executive accountable to the legislature.

The presidential assault on accountability has been long-running. Orman cites episodes of the nineteenth-century presidencies and the succession of contemporary presidents from Franklin Roosevelt onward. Active-positive presidents, the finest accolade among Barber's presidential categories, have all transgressed accountability. Presidents have done so through an almost infinite expansion of the concept of "national security," which has led them to initiate wars unilaterally and even to use the intelligence apparatus for domestic security. Franklin Roosevelt was an early applier of the concept of national security to "internal" or domestic security in commanding the FBI to watch for fascist and communist subversives in domestic society. Subsequent efforts at controlling intelligence agencies by special commissions and congressional committees have had only brief success. The Reagan administration relentlessly promoted such antidemocratic practices as excessive secrecy and deception, media manipulation, anti-civil libertarian acts, presidential war making, and arrogance, all adding up to a formidable total of lawless, unaccountable executive behavior. Orman's study and analysis of the Reagan assaults upon democracy are the best that I have seen.

A growing phenomenon of the unaccountable presidency is celebrity politics, whose contemporary manifestations were first seen in the Kennedy administration. Politicians and entertainment stars have become interchangeable magnets of television attention, with the celebrity loved and chronicled and no detail too trivial for attention. Politics has shrunk in meaning and scope, and truth and reality have become victims of make-believe in the hands of Hollywood and advertising managers.

All this leaves accountability, an indispensable element of democracy, in desperate and still-declining shape. Every traditional contributor to presidential accountability is falling short—Congress, the media, and the public. What can be done to bring democracy back from the abyss of the unaccountable presidency? With the grim trends reported in this volume, the question is surely among the more urgent of U.S. political science.

One response to the problem of accountability is for political science to develop, better than it has, a democratic model of the presidency. At best, this exists in a fragmentary state compared to the more dominant model of the president as a persuader and astute manager of reputation. A fully devised democratic standard could become an evaluative yardstick of presidential performance. It also could be among the undergraduate lesson plans of political science. Presidents and their key aides often seem appallingly ignorant of the nature of democracy, the roles of Congress and the courts, the nature of the Constitution. Unless those who govern in a democracy know and understand the rudiments of democracy, democracy may perish.

The institutions that interact with the presidency must somehow be rallied to perform better in maintaining presidential accountability. Certainly, the odds are formidable. As studies of Congress disclose, legislators give only a lesser priority to overseeing the executive, since they are preoccupied with their own reelection and find lawmaking both personally and politically more satisfying than scrutinizing executive performance. The public gives one little cheer, distracted by trivia in the age of celebrity politics, by television sound bites, and the decline of readership of print journalism. The courts work by ground rules and a style that closely limit their role as enforcers of accountability.

As Orman notes, if Reagan had headed a parliamentary democracy, his administration would have been overthrown after Irangate. While a parliamentary system is not in our future, perhaps some of its devices (e.g., the question hour and the vote of confidence), previously considered and rejected, might well be reconsidered. Presidencies might profitably be studied on a comparative basis. For example, why does one presidency perform well, or largely so, by the criterion of accountability, while another is a disaster? Comparison would provide the more concrete guides that must be formulated if contemporary presidents are to be held accountable.

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The American Speakership: The Office in Historical Perspective. By Ronald M. Peters, Jr. Baltimore, MD: Johns Hopkins University Press, 1990. 354p. $39.50.

As the book's subtitle implies, Peters offers an account of the speakership of the House of Representatives from its inception in 1789 to the present. Congressional scholars have increasingly turned to history in recent years and some excellent, historically informed treatments of congressional leadership have appeared. Still, the most recent comprehensive account of the single most important leadership office over its entire history was Mary Follett's book first published in 1902. Thus, Peters's work is a welcome addition to the literature.

Peters's aim is to "develop a historical understanding of the speakership" (p. 4); he argues that the office "has undergone several distinct historical transformations, and . . . these transformations have paralleled and been caused by broad changes in the political system of which the House and the speakership are a part" (p. 4). Peters posits four speakerships: the parliamentary speakership (1979–1860), the partisan speakership (1861–1910), the feudal speakership (1910–61), and the democratic speakership (since 1961). Policy agenda, party system, and internal institutional arrangements differ in each of these periods and, along with the characteristics and actions of individual speakers, produce these distinct speakerships. A preeminent or defining role characterizes the speakership during each period, Peters contends: presiding officer; hierarchical leader; broker; and therapist, policy maker, and symbol—respectively. Peters devotes one chapter to each of the first three periods and two to the current period. In each case, he describes the speakership and attempts to delineate the historical trends and events that shaped it.

In political science generally and congressional scholarship specifically two very different approaches to understanding and explanation are identifiable. At one extreme are the modelers, who focus upon a few variables assumed to be key, (ruthlessly ignoring others) and make often-heroic assumptions that allow the derivation of clear and frequently testable propositions; on the other are scholars, who, taking their cue from historians rather than economists, attempt to include all possibly relevant variables in their analysis and to explain phenomena in all their complexity.

Peters's approach is near the second pole. A self-conscious exponent of the comprehensive historical approach, he argues that it yields superior understanding to a more parsimonious and rigorous analytic strategy. Thus, while he largely agrees with Cooper and Brady that institutional context is the major determinant of leadership style, he contends that "the 'contexts' that drive the 'style' are themselves a part of a pattern of political and social evolution that must be studied in order to understand the speakership. At the same time, within the contexts, individual actors and events that are not historically or contextually determined will influence the House. An adequate understanding requires narrative description as well as an account of trends" (p. 4).

Peters's study exemplifies the strengths and the weaknesses of this mode of analysis. The study is rich in historical detail but often imprecise and lacking in rigor. We learn a good deal about the historical context and the events of specific speakerships. However, concepts are never clearly defined; and the relationships among the very large number of variables posited as influential are only vaguely specified at best. The author's attempt to be inclusive, to get at the "patterns of political and social evolution" that influence internal contexts sometimes leads him far afield. Thus, his attempt to argue that the post-1961 democratic speakership is, in part, the result of a "therapeutic" political culture is unconvincing. The problem is that this is one of too many critical variables, and he simply does not have space to present evidence and arguments substantiating the existence of the "therapeutic" culture.

In sum, this is a useful book for those interested in the speakership. However, for those who believe precision and rigor are more essential to explanation than richness, it is not the definitive account of the development of that key office.

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The First Amendment, Democracy, and Romance. By Steven H. Shiffrin. Cambridge: Harvard University Press, 1990. 285p. $29.95.

Steven Shiffrin argues against formulas in the application of the First Amendment speech clauses as they have been arrayed against what the courts perceive to be a compelling, substantial, or important governmental interest. In a closely reasoned analysis of case law Shiffrin deals with the difficulty and, indeed, the impossibility of applying the formulas of Melville Nimmer (speech as content v. speech as conduct), Geoffrey Stone (content-based v. content-neutral speech), and Alexander Meiklejohn and Harry Kalven, Jr. (political v. nonpolitical speech). The analysis is supported by impressive and useful content-laden endnotes. Instead of formulas, Shiffrin favors "ad hoc balancing" because "standards of review do not bind those who do not wish to be bound and do little to guide those who wish to be led" (p. 32), although he fails to acknowledge that according to his own contention this is what the courts already do.

As such, this is an excellent work on the subtleties of applying the First Amendment speech clauses to real cases. It is written from the point of view of one who is a civil libertarian and who advocates the efficacy of dissent as a fundamental social commitment.

However, when (in two-thirds of the book) Shiffrin attempts to incorporate this commitment into the larger concerns of political philosophy, the argument becomes substantially weakened and confused. Shiffrin wants to propound that dissent is what defines "American democracy." Beyond this, a focus on concrete situations, as opposed to a rationalist penchant for universal principles, as represented by Emanual Kant and, more currently, John Rawls, Ronald Dworkin, and Robert Nozick, is "romantic." Thus, following Gordon Wood, Shiffrin affirms that the U.S. Constitution was the last great U.S. expression of rationalism and that it was succeeded by a "flexibility" that reflects a "romantic" understanding "and the end of the belief that the endless variety and perplexity of society could be reduced to a simple and harmonious system" (p. 140).

To support all this, Shiffrin adopts a style wherein lists of possible meanings of *democracy* and *romanticism* are advanced and one of the meanings rather arbitrarily employed. The result is a looseness with the language of political philosophy that, one suspects, Shiffrin would not tolerate when dealing with First Amendment applications, a looseness that is also evident when he uses *freedom, rights,* and *liberty* as interchangeable conceptions. Not that Shiffrin is totally unaware of what he is doing. Noting that many political terms "have been used in many different ways by many different people," he nonetheless insists that "intellectual discourse would surely have been less rich and less energetic if these terms had been purged from the scholarly vocabulary" (p. 140). But the point is not to purge the language. It is to figure out what the common core of its meaning has historically been. Only where there truly is no such core is a writer free to be as cavalier with *democratic* and *romantic* as Shiffrin is in this volume.

Agreeing with Meiklejohn that the Constitution is "a reflection of our commitment to self-government" (p. 47) and finding that "majority rule . . . cannot be squared with the original understanding of the Constitution or with modern constitutional practice" (p. 58), Shiffrin contends that the speech clauses of the First Amendment are not really protected by the sorry record of the Court and by "the undemocratic character of judicial review" (p. 98), where the First Amendment has "narrow legal scope" but by an ideological adherence to dissent that "affects the consciousness of the entire society" (p. 88). Accordingly, the meaning of U.S. democracy is found in the value of dissent because this, in fact, is what citizens value. One can, of course, wonder how strong this value is, as Shiffrin himself does at one point where he approvingly quotes Tocqueville's questioning whether the U.S. people are capable of "democracy" (p. 78), while implicitly banishing to the early nineteenth century the possibility that they are not. He also appears oblivious to the idea that judicial review, as the final statement on the meaning of the Constitution, is an inherent violation of constitutional supremacy.

In any event, an emphasis on dissent is part of a larger liberal formulation that is plainly not rooted in the literal egalitarianism of the democratic position. The nearest Shiffrin comes to actually dealing with democracy is when he notes that "some argue that . . . participation is valuable for self-realization or, even more strongly, because it is constitutive of what we are, that is, social and political animals." Yet "leaving these important considerations aside" (p. 67), Shiffrin goes on to proclaim that democracy is something else entirely. He would probably attribute the self-actualization argument, as he does majority rule, to "populist democracy," suggesting that writers who need adjectives to establish their support for something they oppose ought to be read with some skepticism.

Democracy is a doctrine of politics that assumes that all are equal in rational capacity and that if all do not agree because their present abilities vary, the only way to make political decisions is to engage in what Edmund Burke disdained as public policy "told by the head." Simply to ignore the fact that since the Greeks, the conception of the demos has something to do with the distribution of power and authority and, by logical necessity, with majority rule, is fundamentally misleading. To brush off the democratic perception that people can realize their human possibilities only as citizens in an egalitarian community is to avoid the central assertion of democratic advocation.

Unfortunately, what Shiffrin is doing to the concept *democracy* will be applauded by many in the profession. As Keith Graham points out in *The Struggle For Democracy* (1986, 7–9), instead of trying to understand the democratic argument, most political scientists decide a priori which systems are "democratic," observe how people in these systems behave, and end up with many "democracies," all of which are different and usually preceded by the name of a nation-state.

This is more than a semantic issue. As Gaetano Mosca points out in *The Ruling Class* (1939), every political system is characterized by a dominant controlling "political formula"; and the most stable orders are those run by a formula that effectively persuades those who are ruled that they are actually ruling. Clearly, the language of governing is crucial to the formula. *Democracy* implies that a majority is in charge and that the ability to influence public policy is equally distributed among the population.

Shiffrin is correct when he contends that the U.S. people do not really trust majority rule. He is also correct when he points out that majorities are largely out of touch with the prevailing political complexities. But instead of concluding that the U.S. people are not committed to democracy and that indeed, in an extensive and complicated society, democracy may be impossible, he prefers to redefine *democracy*. If, as Shiffrin himself proposes, majority rule may not be feasible or even desirable, it would be refreshing to follow Jefferson and to stop labeling the United States a "democracy." To become ensnared in the term is to encourage a distorted comprehension of the actual arrangements of power and authority. It is to allow a "love-word" to lead to a myopic perception in what is, precisely, an Orwellian manner.

The same semantic games are played with romanticism. Failing to perceive the politics of a world seething with romantic sentiment, Shiffrin uses Ralph Waldo Emerson to demonstrate that romanticism is concerned with concrete situations instead of universal principles. Using *rationalism* as a substitute for formulas, Shiffrin never explains why reason cannot be applied to concrete realities. He also misses the fact that at the bottom of all romantic argument is an epistemological position holding that truth is discovered not through the mind, the senses, or the scientific combination of mind and senses but through the feelings. When Shiffrin propounds that the United States needs a romance with free speech, it might be noted that the history of romanticism in politics historically leads to the kind of emotional nationalism that has produced Nazism and fascism and not to a great deal of respect for First Amendment liberties.

Nevertheless, arguing that "romance celebrates difference" (p. 156), Shiffrin makes his ultimate case for dissent. He might have argued that dissent may influence power and authority and may work to move the world if not in a democratic direction, then at least toward a more egalitarian pluralism where power and authority are less centralized. But because Shiffrin does not address the issue of power, such an argument is not found in this book. What is found is a pronouncement that finally denies the possibility of theory, if the purpose of theory is to make the world less arbitrary and subject to whim, except to the extent that such a pronouncement may be classified as theoretical in itself. Plainly, as Freud well understood, feelings are human; and Shiffrin would like to introduce more human substance into the formulations of constitutional law and the legal establishment. But Freud further understood that behind what appears to be unreasonable there is always a reason.

What the reader ends up with is a very useful volume on efforts to employ the First Amendment speech clauses in specific situations that becomes increasingly murky, and even silly, when the author tries to paint these considerations onto the more expansive canvas of political philosophy. It reveals that while Shiffrin has thought a great deal about the meaning of freedom of speech and the problems of its application in actual cases, he has thought far less deeply about political philosophy. The consequence is a reaching for a perspective that never quite becomes coherent.

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The Logic of Lawmaking: A Spatial Theory Approach. By Gerald S. Strom. Baltimore: Johns Hopkins University Press, 1990. 149p. $30.00 cloth, $14.95 paper.

This book is designed to introduce students of legislative behavior to the spatial theory of legislative decision making. Written at an elementary level and almost entirely nonmathematical, the author introduces students to the basic elements of spatial theory, including preference curves, indifference contours, ideal points, and all the other building blocks familiar to students of spatial voting theory. Beginning with the unidimensional spatial model, the author illustrates Black's median voter theorem, single-peaked and nonsingle-peaked preferences, and the problem of cyclical majorities. In the following chapter he discusses sincere and sophisticated voting and illustrates these concepts with both hypothetical and real-world examples. He then proceeds to the multidimensional spatial model and describes the McKelvey-Schofield instability results. In succeeding chapters he seeks the causes of legislative stability, examining the hypothetical effects of agenda control and the order-producing effect of rules and procedures and finally looks to a general solution concept, the uncovered set, as a prediction of legislative outcomes.

The student who knows little or nothing about spatial theory and would like to learn some of the basic ideas used in this approach will probably be satisfied with this book. Those who wish to know what is currently going on in this field will not be well served, since most of what the author covers dates back to the 1970s and early 1980s. The work of Baron, Ferejohn, Austen-Smith, Banks, Gilligan, Krehbiel, and others of the "games-and-information" school is left out. Also omitted is Ordeshook and Schwartz's (*American Political Science Review* 81 [1987]) article showing that sophisticated voting need not produce outcomes in the uncovered set when the agenda is asymmetric. Their results seriously affect the utility of the uncovered set as a predictor of legislative outcomes.

(Several minor mistakes in the book should be pointed out. The preference curve on page 15 is not single-peaked, contrary to the author's assertion. A single-peaked curve can be flat only at its top. Also, on page 66, it is incorrect to say that Plott's conditions must be satisfied for a majority rule equilibrium. They are sufficient but not necessary. Also, there appears to be something wrong with Figure 5.2 on page 80. The outcome Q should be located to the right of M.)

All in all, the author is careful and writes clearly in a way that is understandable to even the most nontechnical reader. I disagree with the series editor that no book on the spatial theory of legislative decision making has appeared "until now—to initiate the uninitiated" (p. ix). This statement ignores Riker and Ordeshook's *Introduction to Positive Political Theory* (1973, last two chaps.), Enelow and Hinich's *The Spatial Theory of Voting: An Introduction* (1984), and Ordeshook's *Game Theory and Political Theory* (1986), which contains substantial material on spatial voting models of committee decision making. Indeed, Ordeshook covers many of the same topics as the present book, including Black's median voter theorem, single and non-single-peaked preferences, the McKelvey-Schofield results, voting one issue at a time, agenda power, the uncovered set, and sincere versus sophisticated voting. While these books are written at a slightly more advanced level, they are also introductory.

JAMES M. ENELOW

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COMPARATIVE POLITICS

Socialism Unbound. By Stephen Eric Bronner. New York: Routledge, 1990. 241p. $49.95 cloth, $16.95 paper.

The collapse of "official Marxism" and of the party and state structures that sustained it has been coming for a long time. The resulting disarray of what passes for contemporary socialism is as pervasive as the need for an influential Left in an increasingly unstable and dangerous environment. It is only a matter of time before analysis, reconstruction, and activity replace the Right's triumphalism and assist in the revitalization of the socialist project. Such work will doubtless be largely theoretical in nature, and Stephen Bronner has made an important early contribution to this formidable task with the appearance of this book.

Arguing that socialism must have both a democratizing project and a class bias at its core, Bronner traces Marx and Engels's extension of political democracy into society but identifies their scientific "teleology" as socialism's Achilles heel. It would combine with the difficulties encountered by socialist revolutions to sabotage the development of a distinctly democratic socialist ethic. The tendency to identify the proletariat's interests with those of a given party or state led to Bernstein's opportunism and Lenin's voluntarism, each of which marginalized socialism's ethical core by subordinating it to tactical calculations and severing it from political practice. The moral thrust of socialism's commitment to political democracy, economic justice, and internationalism dried up. When World War I destroyed Kautsky's "orthodox" belief that the natural development of capitalism would necessarily produce a revolutionary situation, working-class parties began to adjust themselves to the present. Socialism became a closed system rather than a critical method; and the resulting authoritarianism, dogmatism, and instrumentalism proved fatal.

If socialism's crisis began with Kautsky's failure to use Kant to do more than excuse his skepticism, Bernstein effectively denied socialism's class base altogether and defined it as an eclectic combination of bourgeois political democracy and modest economic redistribution. Revisionism developed an interest in the survival of a capitalism whose stability appeared permanent. Its "socialism of civil servants" rejected the claim that the movement had to rest on the industrial proletariat and reduced it to little more than pluralism. Theory and ideology faded in importance as the proletarian "ethic" took a back seat to the short-run interests of the party in the existing social order. Socialists were transformed into lobbyists.

If Bernstein was the product of advanced Europe's temptations, Lenin was the revolutionary response to oppression in an underdeveloped society. His "vanguard party" anchored his worldview; and the real difficulties of the Russian Revolution merged with an increasingly closed and mechanical Marxism to substitute the party for the class as the decisive emancipatory agent of history. The instrumentalist logic that guided Lenin as he sought to defend a beleaguered revolution led to the systematic elimination of all countervailing institutions and left the Bolsheviks "ethically rudderless" as counterrevolution, isolation, and backwardness doomed the democratic potential of the Soviet republic.

Bronner's preferred "third path" is the "underground" tradition represented by Rosa Luxemburg, a tradition whose heart is the refusal to identify the interests of the working class with any particular party, organization, or set of institutions. This tradition draws its inspiration from the mass strikes of 1905, the Kronstadt rising, the Spanish Civil War and the student uprisings of the 1960s. Bronner's evocation of Luxemburg is meant to restore socialism as a normative historical method and a democratic movement whose task is, and must always be, unfinished. "Libertarian Bolshevik" and "radical social-democratic," the Luxemburg tradition restores democratic politics to Marxism's center as it poses the liberatory and ethical alternative to Bernstein and Lenin. Its openness can help reestablish Marxism as a "critical theory with positive intent" and recreate a partisan socialist ethic that serves democracy as it expands the area in which people shape their lives.

This is Bronner's most important and interesting contribution. His project is to make working-class political power more democratically accountable, to restrict its arbitrariness even as it serves a particular set of partisan policies. Fueled by the "class ideal," socialism must grapple with the old dilemma of coordination and centralization even as the lessons of the past demand local participation and democratic accountability of representative institutions. Bronner pays some attention to the "new social movements" but manages to avoid the anarchosyndicalism of so much of the soi-disant academic Left. Socialism's future is open and contingent. Only if it remains politically, ideologically, and organizationally oriented to democracy, equality, and solidarity can it contribute to the discovery of a new "logic of accumulation." This is an interesting, sophisticated, and deeply principled examination of socialism's past and future and will appeal to readers with a theoretical bent and an interest in its emancipatory project.

JOHN EHRENBERG

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Culture and Conflict in Egyptian-Israeli Relations: A Dialogue of the Deaf. By Raymond Cohen. Bloomington: Indiana University Press, 1990. 208p. $27.50.

Raymond Cohen's thesis is as simple as it is far-reaching: the most important factor that kept Egypt and Israel at war for 30 years and has prevented their "cold peace" from warming up is "a cultural chasm. . . . Neither was able to make itself understood to the other or make sense of the other's equally futile attempts at communication" (p. 7). He admits that the Arab-Israeli conflict is based on more than just misunderstanding, that tangible conflicts of interest are involved. However, he asserts that only the incompatibility of Israeli and Egyptian cultures, manifested in their negotiating styles and inability to read each other's political signals, can explain why the specific disputes of the bilateral Egyptian-Israeli relationship took so long to solve. His analysis rests largely on what he sees as missed negotiating opportunities in two periods (the first from the Free Officers' coup of 1952 to the Suez War of 1956, the second from the end of the War of Attrition in 1970 to the October War of 1973) and on the laborious negotiations following the October War and culminating in the 1979 Egyptian-Israeli peace treaty.

The good intentions of Cohen, who teaches at the Hebrew University of Jerusalem, are manifest. He wants to promote better Egyptian-Israeli relations. He is much more critical of Israeli failures to appreciate what he identifies as Egyptian cultural requirements in political dialogue than he is similar Egyptian deficiencies regarding Israeli culture. The book ends with a high-minded plea for cultural education aimed at mutual understanding. However, his analysis is characterized by two major problems that are common to efforts to explain international disputes solely in cultural terms.

The first and most glaring problem is in defining *culture.* Cohen contends that Egyptian culture is still "village culture," despite the increasing urbanization of the country. "Village culture" is collectivist rather than individual and "high-context" rather than "low-context." It revolves around notions of face, shame, honor, and revenge. Nonverbal signals are more important than words, which are instruments of hyperbole and socially convenient lies rather than conveyers of meaning. Individual human life is not as highly valued as in other cultures, which Cohen proves by comparing homicide statistics in Egypt and Israel. (New York City is by this standard the world's largest village.) One chapter is devoted to the comparison of the Hebrew and Arabic languages, though the author cites no sources actually written in the latter. To support his assertions he relies on a number of standard orientalist texts and a few attitudinal experiments reported in psychological journals. Cohen appears unaware of the major debate in Middle Eastern studies during the last decade over the dangers of cultural stereotyping arising from the epistemological biases of many of the sources he cites as authoritative.

This broad-brush portrait of Egyptian and Arab culture (the two are frequently conflated) leads the author to some egregiously erroneous generalizations. One example will suffice: "The absoluteness of the ruler's executive power over his subjects was never challenged in Moslem society" (p. 28). Islamic opposition movements in the Middle East, including Egypt, would disagree, as would the late shah of Iran. While Cohen seems intuitively on firmer ground in his discussion of Israeli culture, his depiction of Egypt is close to caricature.

The second major flaw in the analysis is the absence of a convincing theoretical link between culture and foreign policy behavior, which is assumed rather than proven. Countervailing tendencies, like the socializing effects of international systemic pressures and the conventions of diplomacy, are ignored or discounted. Culture becomes so encompassing that it can explain everything and its opposite (Egypt's decisions to go to war with Israel and to make peace, Israel's heartfelt desire for peace and intransigence in negotiating it). The domestic political pressures on leaders generated by Israel's parliamentary democratic system are defined as part of its "culture." The argument becomes impossible to falsify. Even the ultimate refutation of Cohen's thesis, the Egyptian-Israeli peace treaty, is explained by the one cultural similarity he detects between the two countries: their acknowledgment of the role of mediators in disputes. (Is there a culture that rejects mediation?) The United States emerges from his account more as a marriage counselor than as a superpower with whom both parties wanted to maintain close military and economic relations even at the cost of concessions in their own relationship.

A focus on more specific factors, such as domestic pressures, the conflicting agendas of politicians, and more general psychological biases that arise from intense security dilemmas and crises would provide a much more convincing explanation for the twists and turns of the Egyptian-Israeli relationship.

F. GREGORY GAUSE III

Columbia University

Hunger and Public Action. By Jean Dreze and Amartya Sen. New York: Oxford University Press, 1989. 373p. $45.00.

Recurrent famines and endemic hunger in Africa and Asia have prompted scholars and policy makers to seek remedies for the extreme human suffering. Sponsored by the World Institute for Development Economics Research, Dreze and Sen have reviewed an extraordinary range of research and provided original data analysis for revealing case studies of India, China, and several African success stories. The authors effectively challenge several widely held perceptions and offer a persuasive rationale for public action grounded on entitlements beyond food relief.

Famine data reveal that millions have often died when food supplies were sufficient but increased prices exceeded the suddenly diminished buying power of marginal consumers. Conventional mass relief efforts involve temporary international food aid rather than more effective locally funded public works employment. The authors convincingly discredit doctrinaire remedies based on a pro- or antimarket ideology. Private distribution mechanisms appear superior to temporary public allocation schemes, but governments must carefully target entitlements to the most needy groups.

The switch from subsistence to cash crop agriculture may either raise or lower nutritional levels depending on other aspects of public action.

Both free market and planned economies have succeeded in reducing hunger (South Korea and Cuba), and both systems have also failed (Brazil and Ethiopia). A free press, civil liberties, and competitive elections are the most effective early warning systems to stimulate government remedial action. Striking differences among countries reveal the significance of both cultural factors and political institutions.

Prior to economic liberalization, China outperformed India on all measures of nutrition, child mortality, morbidity, and life expectancy but still suffered a disastrous politically exacerbated famine after the Great Leap Forward. India, by contrast, has had no mass famine since independence, largely due to the success of public works employment. The female–male ratios in both Asian countries demonstrate extraordinary gender discrimination in food allocation, unlike the equal food entitlements found for women in Africa. Case studies of four African countries that successfully raised nutrition levels and averted famines experienced by neighbors further supports the authors' case for public action to stimulate private effort: "In many circumstances, the spade is a more powerful tool of famine prevention than the spoon" (p. 161).

From the perspective of development economists, malnutrition is closely linked to education, health care, and economic growth. The collapse of communist political systems has immeasurably enhanced the appeal of free market development strategies. The authors nonetheless demonstrate that several impoverished countries have effectively used government intervention to reduce hunger. Their careful empirical analysis demonstrates a complex interplay of problems that defy simplistic, ideological approaches. In keeping with their carefully reasoned methodology the authors recommend no uniform solution but instead identify a variety of government initiatives that can be adapted by local officials to improve income, literacy, and health care in ways that significantly extend life expectancy.

The findings should contribute to the debate in the human rights literature over a "right to food" and the relative priority of political and civil rights. While the authors demonstrate the importance of democratic institutions, dictatorships in Chile, South Korea, and Cuba have more effectively relieved hunger than several representative governments. Successful self-help efforts among many Third World countries also undercut those who insist that the right to food imposes a duty solely on industrialized Western nations. More importantly, the focus on effective public action rather than human tragedy offers valuable guidance for economic planners and struggling with the paramount policy issue of the 1990s.

HOWARD TOLLEY, JR.

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Freedom under Thatcher: Civil Liberties in Modern Britain. By K. D. Ewing and C. A. Gearty. Oxford: Clarendon, 1990. 305p. $45.00 cloth, $11.95 paper.

Ewing and Gearty, law professors at the universities of London and Cambridge, respectively, are concerned about the present state of civil liberties in Britain. While much of their study consists of a case-by-case and statute-by-statute descriptive analysis, it is also an angry critique. Their central argument is that civil liberties have been seriously weakened, especially under Thatcher in her government's determination to see its political programs carried out.

The authors support their contentions with descriptions of the Thatcher government's responses to various expressions of dissent, such as its success in using a nationally coordinated police effort with specially trained units to overcome opposition (notably at Greenham Common) to the placing of U.S. cruise missiles in the United Kingdom, and in breaking the miners' strike of 1984–85, relying heavily on sympathetic judges granting bail to arrested strikers on the condition that they not become further involved. At the same time, the authors describe certain failures the Thatcher government experienced, such as its inability to prevent the exposé of alleged security service misdeeds chronicled in *Spycatcher* from being published, albeit not in Britain.

There is much more, including lengthy treatments of various laws put through by the governments of the day in recent years, again principally under Thatcher, extending police and security service powers—such as lengthened periods of detention following arrest and authority to engage in wiretapping—in dealing with dissent, subversion, and terrorism, especially in Northern Ireland, where warrantless arrests are routine and where the silence of the accused can be used against them at a trial. Throughout the study the courts are portrayed as sympathetic and supportive, Parliament as largely excluded and ineffective. There are frequent comparisons to civil liberties in the United States, Canada, and Australia, to which the British situation is contrasted unfavorably: no written bill of rights, a press unwilling and unable to perform its watchdog function, no meaningful judicial review.

Well-written and carefully researched, the book is useful for several reasons. For students of civil liberties in the United States, it can serve to show how robust U.S. practices are; for students of U.S. politics in general it should be a reminder that beyond a common language and a shared political heritage lie some fundamental institutional differences. Whether Britain is in danger of becoming, in Lord Hailsham's words, "an elective dictatorship," is a warning Ewing and Gearty take seriously; but it needs a lot more study. For Marxists, there is considerable support in the book for the proposition that liberal democratic values are inseparable from liberal social and economic values, so that dissent should be permitted only insofar as fundamental challenges to the established liberal democratic order are not expressed.

The general perspective of the work is typical of many critical characterizations of this period of British politics. For her part, Thatcher on various occasions freely admitted her determination to use the powers at the disposal of her government to pursue nationalist and liberal economic goals and to trim and subordinate civil liberties as necessary to achieve them. As time passes, we will be in a better position to judge her legacy.

LAYNE HOPPE

Texas Lutheran College

Making Democracy in Spain: Grass-roots Struggle in the South, 1955-1975. By Joe Foweraker. Cambridge: Cambridge University Press, 1989. 289p. $44.50.

The Making of the Basque Nation. By Marianne Heiberg. Cambridge: Cambridge University Press, 1989. 266p. $49.50.

While analyzing the "making" of different yet related phenomena, Joe Foweraker and Marianne Heiberg closely agree that "grass-roots" behavior is a fundamental ingredient of macro social and political change. They employ the perspectives, concepts, and research techniques of social anthropology to help explain the laying of foundations for liberal democracy and the birth of ethnic nationalism. Their work gives us unique views of "everyday" society in different regions of Spain and keen insight into the origins of significant influences on social and political affairs in Spain.

Foweraker studies the early activities of the Workers Commissions in the southern Andalucian sherry-producing region around El Marco de Jerez. Analytically, his central contention is that participation in the labor movement through the Workers Commissions provided a critical instrument for the development of a participatory civil society under authoritarian rule. Moreover, in a manner that challenges much of the existing scholarship on the Spanish labor movement, he asserts that the Vertical Syndicate—Franco's corporatist labor institution—provided an early legal structure through which the commissions came to flourish. Foweraker argues that "the commissions succeeded in colonizing the Syndicate, and, in particular, its representative committees" (p. 241). This strategy was central to the making of democracy in Spain, according to Foweraker, because the infiltration and cooptation "was organic, and not rational, willed, or doctrinaire" reaching "across the legal-institutional terrain linking civil society to the State" (p. 262). Foweraker thus contends that the Franquist Syndicate unknowingly served the democratic struggle since the labor movement came to gain "representation" through it.

Heiberg's first seven chapters are a traditional, macrohistorical, description of key leaders, events, and ideas central to the emergence of Basque nationalism in the 1890s and early twentieth century. In describing such things as the ideological work of Sabino de Arana, the founder of Basque nationalism, she clearly and correctly considers the intertwining role of nationalism, industrialization, immigration, and labor activity. She argues that Basque nationalism began as an urban phenomenon, only later moving into the countryside. It reflected an intense conflict between rural and urban culture, not just Basque against Spanish culture. The "people," particularly the middle and lower-middle classes in Bilbao, were pitted against "outsiders": non-Basque socialist workers and miners and the small, powerful Basque industrial oligarchy with its advocating of liberalism (p. 50). This early nationalism emphasized the urban intellectuals' turn to a utopian past, rural culture, and the Basque language and symbols. They praised traditional and precapitalist Basque society with its egalitarianism and democracy, peace and social order, and Catholicism in contrast to the anti-Basque values of modernization and industrial society. Only later, in opposition to Franco's centralism, did contemporary nationalism take a socialist and secular turn.

In the more original last four chapters, Heiberg analyzes life in the small village of Elgeta in Guipúzcoa. Based on participant observation, she argues that the social system, moral codes and values, and the power structure at the local level are central to an understanding of the development of Basque nationalism. Moral codes and values in particular, she suggests, served as organizing agents to form a collective Basque conscience, which included the concept of the pueblo, the ideology of equality, daily life based on trust, social interaction and identity through groups, and a self-perceived status of Basque "nobility." Education and the Ibastola (the Basque language school), the factory cooperatives, the Church, and Basque fiestas as a means of cultural affirmation institutionally strengthened this collective conscience. Reinforced by the power of social and local economic sanctions, this "moral community" helped nationalism take root.

Characteristic of the social anthropological approach, these two books are strong on description and rich in detail. Each highlights the vast diversity of Spain's regions through the study of the local cultures, ideologies, and economies. For example, Foweraker's precise description of the cultivation of the vineyards and the politics behind them should be enjoyed by any scholarly lover of good sherry. Similarly, Heiberg's discussions of the local elections of 1967, the story of local electoral manipulation (p. 188), and the personalized nature of political groupings in the village make for informative political reading. Also characteristic of social anthropology, both books strongly emphasize the role of informal personal networks. These grass-roots networks proved essential under authoritarian conditions for communication fostering social bonding and the development of participatory civil society. In the Basque country they strengthened nationalistic values, while in El Marco they communicated the immediate needs of the workers, producing in an evolutionary fashion the commissions' syndical strategy.

For political scientists, the strength and usefulness of these works may partly depend on one's affinity to the social anthropological approach. The rich description illuminates the primacy of the informal, rather than the formal, social and political institutions. Undoubtedly, such insight adds significantly to our understanding of nationalism and early labor behavior. To the more skeptical reader, however, the presentation of so much detail raises several research questions. For example, the "typicalness" and the criteria for selection of Elgeta and El Marco de Jerez merits some justification. Foweraker's statement that "El Marco de Jerez should be seen as a microcosm of Spanish civil society writ large" (p. 47) leaves open questions about how representative such micro behavior was of political attitudes and behavior throughout the country, particularly given the regional and ethnical diversity of multinational Spain. Furthermore, questions about the role of theory are brought out by the different organization of the two books. To the unfamiliar reader, Heiberg's movement from a general overview of Basque history to the specifics of life in Elgeta may flow better and appear better organized, given that the background comes first. However, Foweraker's desire to tell a "story" (specific to general) is more consistent in quality.

More important, both books leave some analytical gaps between grass-roots participation and elite political behavior during Spain's rapid political change. For example, while useful at giving insight into the ideological conflicts surrounding and within the Basque nationalist movement, Heiberg is better at describing the social aspects of nationalism than the political turns it has taken. Her heavy reliance in the first section on previously published works (e.g., by Stanley Payne and other historians) is partly responsible for this. While interesting, the first section breaks little new ground. More harmful, Heiberg's book was unfortunately not fully updated prior to publication (it was originally a dissertation), though the well-thought-out and rewritten conclusion is a pleasant exception. The majority of the field research was completed in 1975 (Franco died in November of that year), so her statements about "recent" events and publications appear awkward if the reader has followed the events of the 1980s; and the bibliography seems to end about 1979. As Heiberg herself suggests in the postscript, democratic Spain is quite different 10 to 15 years later. Thus, this book should be read by readers interested in sociological aspects of Basque nationalism; however, those wishing to know more about the role of Basque nationalism in Spain's transition to democracy may find other works more useful.

Foweraker, on the other hand, attempts to link grass-roots participation and elite behavior by interspersing chapters of his social anthropological description and analysis of affairs in the broader labor movement, e.g., his excellent discussion of decisions leading to the commissions' strategy of infiltration (chap. 8) and of corporatism (chap. 14). Yet exactly *how* the pressures from below—those created by the participatory civil society in places like El Marco de Jerez—produced the elite behavior that directed Spain's transition to democracy could have been more fully developed. Furthermore, better attention to the exact nature of these pressures would have exposed Foweraker's more serious problem at the lower end of the grass roots–elite relationship. Here Foweraker never addresses precisely what he means by democracy—something more than a matter of semantics, given the book's focus on the "making of democracy." While most other works on the transition focus on "liberal democracy," Foweraker makes only indirect statements about democracy in a populist sense. He seems to equate "popular" and "democratic" struggles. Populism, however, can and often has taken some very harsh and nondemocratic forms. Foweraker obviously believes that democracy is about people; but how this related to liberal democracy could have been more directly addressed. Nevertheless, Foweraker's work should be read because of his belief that participatory behavior ultimately made a difference in the making of democracy, even if his notion of "participatory" democracy sits awkwardly next to the existing literature based on survey data, which suggests a highly apolitical or politically apathetic population in much of Spain during this period.

Subsequent research should pay more attention to the Foweraker book. This stems, in large part, from his controversial stands, his confrontational style, and his more rigorous methodology and questioning. Foweraker strongly challenges the literature directly in several areas, while Heiberg tends to accept previous research without question. For example, Foweraker argues the Workers Commissions first appeared in El Marco de Jerez around 1953 or 1954, not in the coal mines of La Camocha in Asturias several years later, as other scholars writing on the labor movement under Franco have suggested (e.g., Fishman, Ibarruri, Miguelez, Maravall, and Mujal-Leon). Foweraker also stirs controversies concerning the nature and development of the relationship between the Spanish Communist party and the Workers Commissions and his belief about the Commissions' infiltration strategy.

In conclusion, numerous books and articles have attempted to document, interpret, and analyze social and political change in Spain. The majority have focused either on elite behavior or, through survey research, on individual-level attitudes. These two books are pleasant deviations from this trend. Despite some shortcomings, their social-anthropological approach captures well the emotional aspects of the local societies on different ends of the Iberian peninsula. They leave the reader both with a good sense of the fear and excitement of grass-roots labor activity during the Franco period and the closed-circle life of Basque society, with its exclusion of outsiders and love of things distinctly Basque. Both these books are thus welcomed additions to the literature on social and political behavior in twentieth-century Spain.

THOMAS D. LANCASTER

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One Land, Two Peoples: The Conflict over Palestine. By Deborah J. Gerner. Boulder: Westview, 1991. 220p. $32.00 cloth, $10.95 paper.

Few of the books on the Palestinian question and the Arab-Israeli conflict are written to serve as undergraduate textbooks. Gerner succeeds in this endeavor by writing a short, well-organized, highly informative, and fairly comprehensive introductory volume. The author traces the evolution of the conflict and places it squarely in the modern phenomena of self-determination and nationalism. Gerner also demonstrates that this conflict between two peoples over the same country early on became internationalized and mired in post-World War I and post-World War II major power rivalries. She has composed the narrative around four main themes: (1) the search for national identity and self-determination; (2) the role of the superpowers, especially colonial Britain and the post-World War II United States; (3) the role of national policies of Israel, the Palestine Liberation Organization, and Arab countries; and (4) the roles of the League of Nations and the United Nations.

True to its undergraduate audience, this book provides useful learning tools: a list of acronyms, a glossary of key terms, maps, discussion questions, gender-neutral language, an extensive bibliography, and photographs. The use of photographs humanizes political questions; therefore, authors ought to be extremely careful and even-handed in their choice of images. Gerner's selection of photographs showed some bias by including one picture of Israeli children, another of an Israeli farmer, but none of a similar character of Palestinian persons. Inadvertently, no doubt, the visual outcome indicates human productivity and vulnerability for only one side of the conflict.

This visual slant is by no means reflected in the book's narrative. Gerner at her best captures parallels in Israeli and Palestinian experiences and shows the complexity that the question of Palestine deserves. Indeed, a central thesis of the book is that Palestinian and Israeli policies are similarly subject to the interplay of domestic and international constraints as parameters that have guided each people's history.

The author thoroughly and painstakingly chronicles all major historical and current events that helped form the political memories of the Israelis and the Palestinians. These include the international commissions, the national conferences and agreements, the partition plan, the popular resistance groups, the development of national leadership, the wars, the roles played by regional and superpower rivalries, and the recent developments of nationalist-religious extremism. Gerner ends the narrative with a discussion of options for the resolution of the conflict, including the highly sensitive issue of the status of Jerusalem.

The author pays more attention to external than to domestic dynamics in the conflict, perhaps because she targets international affairs students. The book curiously lacks treatment of the topic of popular mobilization, especially the interplay of strategies and resources that typically colors nationalist movements. Gerner gives a brief introduction to key Israeli and Palestinian political organizations, including the politically active religious fundamentalists; however, she awards other significant domestic groups only passing mention.

Curiously absent is discussion of the political contributions of women. The Jewish women's organizations, such as Hadassah and the Palestinian women's committees and societies, have played important mobilizational and social welfare roles in their respective people's nation building. Social welfare activities that have formed part of the essential framework of both Palestinian and Israeli national development are exemplified by the Israeli Histadrut labor organization and the PLO's Families of the Martyrs Institution. These and other social welfare organizations have served the important function of softening the sharp edges of these struggles for self-determination and nation building.

Reading Gerner in the aftermath of the Gulf War is bound to produce much speculation about the effects of the changing military and power arrangements in the Middle East and their impact on the question of Palestine.

Regardless of future developments, though, this brief introductory book will remain valuable as a historical overview. This well-written analysis that will find use as a supplementary text in introductory courses in the fields of international relations and comparative politics and, certainly, in courses on Middle East politics and the Arab-Israeli conflict.

AMAL KAWAR

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The Lutheran Church and the East German State. By Robert F. Goeckel. Ithaca: Cornell University Press, 1990. 327p. $39.95.

Catholicism and Politics in Communist Societies. Edited by Pedro Ramet. Durham, NC: Duke University Press, 1990. 454p. $49.95 cloth, $21.95 paper.

It is commonplace of late to note the impact of religion and the church upon the polity. Ever since the rise of Solidarity in Poland and the surge of militant Islam in the Arab world, the more popular observers have noted, sometimes with alarm, the power of religious belief and organization to sway—even to destroy—public policies and governmental institutions. Fortunately, a more scholarly literature has also come to the fore during the same period, with studies offering a more eloquent analysis. Two good examples are found in these two works.

Goeckel's study is eloquent, as well as penetrating. Goeckel analyzes the relationship of the Evangelical Lutheran church with the state and the Marxist-Leninist party in East Germany and explains how the two could come to a working relationship in spite of decades of ideological antagonism. His work is historical-analytic in approach, and the historical presentation is authoritative in its detail and analysis.

Ramet has edited and significantly contributed to a large work examining the relationship of the Catholic Church to the party and the state in the Soviet Union, East Germany, Poland, Czechoslovakia, Hungary, Yugoslavia, Romania, Albania, China, Vietnam, Cuba, and in Sandinista Nicaragua. Again, the approach is primarily historical-analytic; but the selections are without exception meaty and often right up to date. It is interesting to note that the well-writen contribution on East Germany is by Goeckel and that the chapter on the church in Albania by Janice Brown correctly presages the recent changes toward religion in general and the relaxation toward the Catholic faith in Albania in particular.

Both of these works are directed toward their own principal audiences. Goeckel's study is for the German scholar, the person interested in what it actually means to live as a believer in a Socialist (Marxist-Leninist) Germany. He shows how improvement in the church-state relationship in East Germany had to take place regardless of ideology or theology, since neither actor was going to leave the scene. Thus, improvement in the relationship took place without legal changes. The whole arena of church-state relations in East Germany took on an extralegal atmosphere: a decidedly un-German state of affairs. Ultimately, as we know from the events of late 1989, this extralegal condition would move into a situation of direct confrontation of the regime by the Evangelical Lutheran Church. His study of why the regime was actually so weak in the face of the church's stand is both interesting and rewarding. For example, his discussion of the "legitimacy deficit," of the role of the international nongovernmental organizations in regard to East Germany, and his analysis of the failure of state mobilization efforts over the decades portray a regime long on parades and iron-fisted rules but short in terms of real support and perceived rightfulness. Goeckel's analysis of the church, especially its weaknesses, is not as sharp as his excellent examination of the regime.

Ramet and his 14 fellow contributors offer a much wider coverage for a wider audience, not focused upon a single political system but rather interested in the condition of the Catholic Church across the world of "socialist" states today. The second in a three-volume series, Christianity under Stress, on the experience and development of Christian churches in communist-ruled societies, it offers an overview rather than a penetrating insight à la Goeckel. The book refutes the idea of the Catholic Church as some kind of monolith and instead emphasizes the diversity and syncretism of the Church (something noted a millennium-and-a-half ago as the Church remolded and wiggled itself around to allow for the inclusion of the barbarian masses and their cultures). Survival in hard-line socialist states today requires the same adaptive ability as faced by the early Church in the dark German forests, so long ago. The contributions are organized around the themes of tradition versus modernity, hierarchy versus lower clergy, and institutional structures as opposed to local organization. The overview that emerges is factual, analytic, and well presented. This big book will be especially valuable for the seminar in religion and politics where the students will find it a treasure trove of fascinating reading.

These works are quite different, Goeckel being a straight line into a single problem area while Ramet is a multipronged examination of a wide-ranging topic. But both offer the scholarship that is so valuable in the exciting arena of church-state relations, an arena that will become more central as the relations become more intense.

GERARD F. RUTAN

Western Washington University

State Politics in Zimbabwe. By Jeffrey Herbst. Berkeley: University of California Press, 1990. 283p. $38.00.

The Last Colony in Africa: Diplomacy and the Independence of Rhodesia. By Michael Charlton. London: Basil Blackwell, 1990. 164p. $37.95.

The institutional arrangements that Zimbabwe inherited at independence were the outcome of 90 years of settler colonial rule, a protracted and internationalized armed struggle, and the Lancaster House conference during which all parties ultimately accepted Britain's proposals for a new constitution, cease-fire, and majority rule elections. While the constitution established many of the broad parameters within which public policy in Zimbabwe would be formulated, international pressures upon all of the participants also played a vital role in determining the outcome of the conference itself. These international pressures form the central theme of Michael Charlton's book, while the analytic task for Jeffrey Herbst is to determine the extent to which the Zimbabwean state is able to make public policy independently of societal forces.

Charlton, a journalist with the BBC, has compiled an "oral history of a few months of remarkable diplomacy" (p. vii) that resulted in Zimbabwe's independence. Charlton interviewed an impressive group, including Lord Carrington and others from the British foreign policy establishment; President Kenneth Kaunda of Zambia; General Olusegun Obasanjo, former head of the state of Nigeria; Sir Shridath Ramphal, former secretary-general of the commonwealth; Pik Botha, foreign minister of South Africa; and Joaquim Chissano, former foreign minister and current president of Mozambique. From Zimbabwe, Charlton's interviewees include, among others, Robert Mugabe, current president of Zimbabwe and head of the ruling party; Joshua Nkomo, vice president of Zimbabwe and former head of the ruling party; Abel Muzorewa, former prime minister of Zimbabwe Rhodesia; and Ian Smith, former prime minister of Rhodesia. Drawing from his interviews, Charlton then stitches together a chronological account of Mrs. Thatcher's about-face on the recognition of the Muzorewa regime, the Commonwealth Conference in Lusaka, the Lancaster House Conference in London, the implementation of a cease-fire and new elections, and the transition to independence.

Perhaps one of the most intriguing aspects of Charlton's work is his examination of how the actions of a host of actors, both within the British foreign policy establishment and from within the commonwealth, contributed to the change in the policies that Mrs. Thatcher was willing to pursue toward Britain's last colony. Equally interesting are the first-hand accounts of how, by threatening to withdraw their support, Presidents Machel, Nyerere, and Kaunda were able to force Mugabe and Nkomo to participate in the Lancaster House process and, ultimately, to accept Britain's proposals.

The work does, however, contain several major flaws. First, Charlton glosses over the land issue, which nearly derailed the entire Lancaster House process. While such an outcome was avoided only after extensive promises of international assistance were made by President Carter, Charlton interviewed no U.S. officials. In addition, while Charlton enables readers to experience the process of decolonization through the words of the critical actors, very little of the information itself is new. Charlton also defines his task very narrowly as an oral history and he makes virtually no use of any of the related works, nor does he attempt to link his work to a wider body of literature through the use of notes or a bibliography.

While Charlton examines the process leading up to Zimbabwe's independence, Herbst focuses his attention upon the government of Zimbabwe and the autonomy with which it has acted since 1980. Politics in Africa is, according to Herbst, the result of the "wisdom of governmental officials using the resources that are available to them" (p. 163). Thus, Herbst proposes to break away from existing studies of state autonomy, which often fail to provide the empirical basis for the grand theoretical claims they make, by examining both the "structural autonomy" of the Zimbabwean state (ability to establish the rules by which policy will be made) and its "situational autonomy" (extent to which societal groups influence the day-to-day allocation of resources) (p. 7). Herbst goes on to argue that in any given issue area, the degree of structural autonomy a state experiences need not be related to its situation autonomy and that the extent of both may change dramatically from one issue area to another. In addition to the nature of the issue area, the "locus of decision making" within the state can also be a major factor in determining state autonomy. By examining how governments actually make decisions, Herbst also hopes to shed light upon the role of parties in Africa (p. 7.).

The empirical component of Herbst's study consists of a series of rigorous case studies of resettlement, squatters, agricultural pricing, foreign investment, multinational mining corporations, and health care. With the exception of resettlement and existing foreign investment—areas in which the broad parameters of governmental action were determined by the Lancaster House Constitution—Herbst finds extensive structural autonomy in Zimbabwe. The fact that for instance, large-scale commercial farmers participate in setting agricultural prices is the result of government decisions to permit such participation. The level of situational autonomy, however, varies widely from area to area and depends upon the "match" between the resources and tactics societal groups possess, the characteristics of specific issue areas, and the locus of decision making.

Herbst argues that the iterated nature of decision making—its repetition on a regular basis—in setting agricultural prices and wages has enabled some groups, such as cotton growers and employer groups, to learn how to lobby the government effectively. In contrast, due to the locus of decision making within the government and their own lack of legitimacy in government's eyes, multinational mining corporations proved unable to marshal their extensive resources to influence the government's decision to take over the marketing of mineral exports. Similarly, Zimbabwe's ability to avoid inter-ethnic rivalries in the allocation of health clinics has stemmed from its decision to decentralize decision making to the provincial level where Shona and Ndebele groups compete among themselves, rather than with each other. In the area of resettlement, the weak administrative capacity of the state to provide the extensive support services that successful resettlement requires, rather than effective pressure from commercial farmers, has limited the state's ability to achieve its own resettlement goals. The creative mobilization of local and national support from the ruling party has also enabled squatters to run roughshod over a state structure that was largely debilitated during 16 years of guerrilla war.

Herbst's work is without doubt one of the most comprehensive studies of policy making in Zimbabwe. The major strength of the study, the rigor with which it treats the relative autonomy of the state from societal groups, is also its major weakness. Herbst focuses upon the internal dimension of state autonomy at the expense of the external dimension. In his discussion of foreign investment, the role of international donor agencies, particularly the International Monetary Fund (IMF) and the World Bank, is hardly discussed, nor are the restraints upon state autonomy that stem from its regional setting. Both of these are of critical importance given the rapid pace of change occurring in Southern Africa and the dramatic policy changes that are taking place in Zimbabwe. At this point, we must still speculate as to the autonomy with which the Zimbabwean state operated in formulating its current structural adjustment program. While the government has claimed that the program is "homegrown," it includes many provisions (e.g., trade liberalization, foreign exchange convertibility, labor relations policies that will make the dismissal of workers easier, and massive cutbacks in the size of the state as well as its role in regulating the economy and in providing social services) that have come to characterize IMF-inspired plans implemented in other parts of Africa. Rather than a shortcoming in Herbst's fine study, analyzing the external dimension of state autonomy in Zimbabwe is the next logical step in the analysis of state autonomy in Africa.

WILLIAM CYRUS REED

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Nuclear Politics: Energy and the State in the United States, Sweden, and France. By James M. Jasper. Princeton: Princeton University Press, 1990. 327p. $39.50.

This book presents a knowledgeable discussion of such technical matters as light water versus heavy water nuclear reactors and boiling versus pressurized reactors. However, the main thrust is an examination of how three very different policy perspectives regarding technical development and the constituencies holding these views interacted to influence civil nuclear policy in three modern states. Further, the author explores how partisan politics on the national level influenced state policy.

Jasper's stated purpose is to add documentation to the view that public policy cannot be understood solely on the basis of the examination of formal political and economic structures. Jasper goes beyond structural analysis to suggest that other factors, such as elite discretion, structural change, social psychology, and cultural meanings play a role in shaping advanced state policy on technological issues. Using as a point of departure Eric Nordlinger's *On the Autonomy of the Democratic State* (1981), Jasper sets out to examine what factors determined the outcome of the pulling and hauling over nuclear policy within three state contexts.

Jasper describes the three different policy approaches to modern technical development policy as being the *cost-benefit*, *technology enthusiasm*, and *moralistic* approaches. Typically, persons holding cost-benefit perspectives are trained in economics. They therefore view policy alternatives through the lens of economic analysis, which seeks to utilize finite resources efficiently in order to maximize a given society's capability for production. For Jasper, the technological enthusiasts are persons who feel most comfortable with physical structures whose behavior can be predicted with reference to the observed laws of nature. For technological enthusiasts the preferred path toward solving social problems is by means of research, development, and construction. The third approach to policy making reflects what Jasper calls the moralistic perspective. Persons in this category believe they can discern principles of right and wrong and that these should guide public policy. In the civil nuclear power context, moralists are perceived by Jasper to be generally environmental moralists concerned about the potential for harming nature inherent in civil nuclear power plants.

Jasper chronicles how the interactions of these three policy perspectives, modified by partisan political views, (i.e., To what extent should government underwrite commercial business activity?) interacted in ways that lead the French to develop a large-scale nuclear power base for their national electric system, the United States to cease building nuclear power plants, and the Swedes to adopt a plan to phase out nuclear power plants in the first decade of the next century.

The methodology used by Jasper is straightforward. One primary research technique was elite interviews. The interview schedule involved one hundred officials from governments and electric utilities, antinuclear activists, and other participants in the policy process. The second basic methodology was archive research.

Jasper concludes the book with a number of theoretical conclusions that will be of particular interest to public policy scholars. Examples are the observations that "the formal power and position of groups and organizations do not fully explain their successes and failures and hence poorly explain political outcomes" and that "explanations of policy outcomes can be improved by supplementing structural factors with cultural meanings. Two sets of meanings that proved useful are dominant partisan cleavages and policy styles." Jasper also advanced five practical conclusions for those who participate in government and the technical communities. Examples of these are that "Veblen was right that there are penalties for taking the lead in many countries" and that "in the case of expensive technologies with long construction lead times, policies are often adopted without much information about the full effects and costs of the technology; in fact, it is only by adopting the technology that the full costs and benefits become clear."

ROBERT M. LAWRENCE

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Beyond the European Left: Ideology and Political Action in the Belgian Ecology Parties. By Herbert Kitschelt and Staf Hellemans. Durham: Duke University Press, 1990. 262p. $39.50.

During the 1980s, a number of advanced Western democracies witnessed the rise to political prominence of a variety of Green, alternative, and left-socialist parties. Despite their frequent claim to be neither left nor right, they represent by now the most serious challenge to the established social democratic, socialist, and communist parties.

Among the most ambitious attempts to explain the emergence of these parties and its implications for traditional left-wing politics has been Herbert Kitschelt's work. In a number of articles and a book-length comparative study of the German and Belgian Greens (*The Logics of Party Formation*, 1989) Kitschelt has shown that what he calls "left-libertarian parties" tend to emerge in corporatist welfare states that were dominated by socialist governments in the 1960s and 1970s. As a result of the rigid policy-making process that disallowed outsider participation in political decisions, social movements opted for organizing themselves into left-libertarian parties. These parties combine two ideological positions: they endorse the traditional Left's call for egalitarian redistribution but reject its tendency towards bureaucratic centralization of political and economic power. Instead they support societal and economic decentralization and broader political participation.

The present study, based on a comprehensive survey of 156 party militants, explores the ideology and organizational structure of the Flemish and Wallonian ecology parties, Agalev and Ecolo. The central thesis of the book is that a party's ideological direction, organizational structure, and political strategy results from "the relative strength and interaction of multiple subgroups of activists" (p. 16). Which subgroup will exert most influence on the party in turn critically depends on the mobilization of the social cleavages and constituencies a party seeks to represent. Strong cleavage mobilization leads to radical visions of social change, declining mobilization to increasingly pragmatic policies.

The authors argue that parties include at least three clearly distinct subgroups of activists: *ideologues*, *lobbyists*, and *pragmatists*. Ideologues tend to have a greater commitment to protest politics and emphasize collective goods while being opposed to piecemeal reforms; lobbyists tend to be affiliated with social movements and are therefore primarily interested in providing benefits to party constituencies; pragmatists, although sharing the ideologues' concern for a universalistic program, tend to see a balanced policy of incremental change as the best way to attain overall social transformation. Associated with these groups are two logics of political strategy. Whereas ideologues promote a "logic of constituency representation" appealing only to the most committed and mobilized supporters, pragmatists follow a "logic of competition," trying to maximize voter support in order to gain political office. Lobbyists finally tend to maneuver between these two poles, offering themselves as coalition partners to whichever group provides most for their constituencies. From this model the authors advance the hypothesis that in areas with high cleavage mobilization left-libertarian parties should attract more ideologues and lobbyists than pragmatists. As a result, a party's ideology should be more radical and its organizational structure more decentralized, democratic, and participatory than where it is dominated by pragmatists.

The empirical part of the study supports this hypothesis. In Flanders, where the left-libertarian cleavage mobilization was much more pronounced in the early 1980s than in Wallonia, Agalev has attracted considerably more ideologues and lobbyists into the party. In addition, ideologues play a significantly larger role in influencing the party's national political strategy than in Ecolo, which is largely dominated by pragmatists. As a result, Agalev has been more radical in its ideology and more pluralist and participatory in its internal decision-making process than its counterpart in Wallonia.

The present study is a valuable contribution not only to the study of the internal dynamics of left-libertarian politics in Belgium but to the study of political parties in general. It advances a wealth of theoretical insights into the logics of internal party competition and policy formation and the implications that follow from strategic choices. This is particularly welcome at a time when prominent left-libertarian and traditional left parties such as the German Greens and Social Democrats have suffered dramatic voter losses. Kitschelt and Hellemans' model would suggest that these losses are attributable to the fact that both parties continued to be dominated by ideologues on the national level promoting a radical strategy at a time of rapidly diminishing left-libertarian cleavage mobilization in Germany. Since this trend is hardly restricted to Germany, it follows that left parties will have to adopt more pragmatist strategies if they want to remain politically relevant actors in the 1990s.

HANS-GEORG BETZ

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Marketing Democracy: Public Opinion and Media Formation in Democratic Societies. By Romain Laufer and Catherine Paradeise. New Brunswick, NJ: Transaction, 1990. 347p. $39.95.

The failure of the marketplace to approximate democratic ideals has occupied the attention of increasing numbers of scholars concerned with public opinion and techniques of mass persuasion. In *Marketing Democracy,* Romain Laufer and Catherine Paradeise, French professors of marketing and sociology, respectively, focus primarily on relating contemporary marketing to the history of reason in Western society. Marketing, they argue, is the bureaucratic form of sophism, and part of a contemporary victory of opinion over reason. The book's ambitious undertaking is "to show that the emergence of marketing has been a major feature in the intellectual and social changes of the contemporary world" (p. ix).

The authors argue that Western societies are rediscovering the theory and practice of persuasive speech and that as recently as 15 years ago, many sectors of the economy such as hotels, banks and manufacturing companies were functioning quite well without marketing. This assertion forms the basis for renewed concern over the societal impact of marketing and its implications for the functioning of a marketplace of ideas. Depending upon how broadly one construes the boundaries of *marketing* and which countries are examined, their timing of this historical transition appears delayed; in the United States for example, the field of public relations was flourishing by the mid-twentieth century. One also has to wonder whether the "rediscovery" of persuasive techniques is not simply a continuation of long-standing practices made more visible by mass media.

The book's title is somewhat misleading; although the history and consequences of the development of marketing techniques are dealt with in some depth, the formation of media are not. Notably absent are discussions of the concentration of ownership of media industries and other related trends bearing on the operation of a marketplace of ideas. And although the authors' arguments are framed quite generally in terms of "democratic societies," they are typically referring specifically to the United States or France. Laufer and Paradeise present an interesting account of the development of marketing in France, but many U.S. readers will find their grasp of the development of U.S. marketing outdated. For example, the idea that one can "sell a politician like a packet of washing powder" (p. 90) will not come as a revelation or surprise. (As has been noted in several recent U.S. elections, the analogy now seems unfair to soap.)

Within these limitations, the book does a good job of outlining the relationship between the rise of marketing and declining confidence in marketplace principles. Growing public awareness of the machinery of manipulation has led to the current crisis of legitimacy. The authors also do a very thorough job of tracing the history of the free market system as a means of legitimizing social power. Although the scope and contours of the current crisis of legitimacy are not fully described, it is argued to result from the corruption of the marketplace from one with a basis in objective need to one based on irrational desires.

Organizationally, the book could have made a stronger case for its arguments if it had made clear *which* democracies the authors are referring to in specific examples. Since it is often difficult to make generalizations about Western democracies, readers may be tempted to challenge the authors' assertions with exceptions that make their general points no less valid. Some awkwardness in the use of language no doubt results from the book's translation from the French, but it is difficult to ignore the use of inappropriate gender-specific language, such as "the marketing men," "the men in political marketing," and so forth.

Marketing Democracy poses important lingering questions about the nature of the relationship between the public sector's adoption of marketing techniques and the legitimacy of political institutions. The authors highlight the power of marketing as "the charmer to whom nothing is denied" and words and slogans as "all-powerful despots" to be used when "the marketing man seeks to mobilize by seduction." While few people today doubt the tremendous power of marketing, one has to wonder why such powerful techniques in the hands of government and political leaders should not result in an extremely effective consensus-building apparatus and a surfeit of political legitimacy, however illegitimate its basis.

DIANA C. MUTZ

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The Crisis of Argentine Capitalism. By Paul H. Lewis. Chapel Hill: University of North Carolina Press, 1990. 573p. $49.95.

Like other researchers over the years, Paul Lewis attempts to understand the Argentine stalemate between liberal and populist economic measures since the advent of early Peronism. Reminiscent of Carlos Waisman's *Reversal of Development in Argentina* (1987), Lewis's study ascribes the basic blame to the creation of a type of "hothouse capitalism"—an economy both heavily and indiscriminately protected and inefficient.

In the introduction to this large volume, Lewis sets out to depict and analyze the Argentine industrial class in the modern era. The book is clearly much more than that, since it also does justice to an overall economic and political history of Argentina between 1910 and 1985. As can happen when a sensitive and knowledgeable political scientist handles economic questions, it is a very readable and informative account of Argentine obstacles to sustained economic growth. However, in the process, the book soon becomes a less-disciplined project, expanding its net to include a historical investigation of the military, the agrarian sector, foreign capital, the unions and Perón's political leadership itself. Thus, as the canvas broadens, we lose somewhat the basic promise of a clearly demarcated analysis of the Argentine capitalist class. However, having said that, we still find that Lewis presents us with the best account in English of the ideology and policies and the very activities of critical sectors of the Argentine bourgeoisie.

Lewis raises various hypotheses for explaining Argentine economic stagnation in his introduction; and by the time he reaches the concluding chapter, it is clear that the Peronist administrations (1943–55) were largely responsible for moving Argentina toward an increasingly ineffectual statist society and economy. A network of highly subsidized governmental enterprises and protected light industries were launched that were more directed to employment considerations than to productivity and international competitiveness. The corporative state that emerged under Perón undermined entrepreneurial potential. Peronism, according to Lewis, has permanently divided the moneyed classes from the workers:

> For [the moneyed classes], the politicians are prehensile demagogues, the military are inept and untrustworthy, and the people have been permanently corrupted by Peronism into preferring plunder to hard work. . . . The people remain unmoved when liberal[s] . . . call for sacrifice and more productivity because they are convinced that the oligarchs will only pocket the profits and not reinvest them. Thus, the perpetual standoff: the people cannot raise their living standards and the oligarchy cannot enjoy its wealth securely. (p. 502)

Thus the pre-Perón potential for sustained and balanced development, which Waisman has also identified, is derailed and never adequately recaptured.

Though there seems to be an overdependence on a single English-language source (the venerable *Review of the River Plate)*, Lewis has done a prodigious amount of reading (including probably every Ph.D. dissertation on Argentina politics and economics in the last half century), culling and synthesizing; and the result is a very good compendium of the trials and tribulations of the entrepreneurial sector vis-à-vis other corporate interests. The best parts of the book include numerous interesting cases and vignettes of key Argentine elite economic families and business associations. Here we find fascinating new information about the thinking and operational procedures of such industrial captains and entities as Bunge, Colombo, Di Tella, Tornquist, Braun-Menéndez, Roberts, Shaw, the Argentine Industrial Union and the General Economic Confederation.

The major culprits, in Lewis's estimation, are the labyrinth of statist, neomercantile regulations that confront entrepreneurial initiatives (which were no doubt reinforced by changing and unstable regimes inhibiting the formation of a predictable investment climate). One is reminded of Hernando De Soto's *Other Path* (1989), portraying a similar Peruvian experience. The chapter entitled "Local Businessmen and Their Limitations" is a perceptive analysis of the get-rich-quick mentality of the Argentine businessman. In the process, Lewis gives us rich, textured material with scores of interesting examples of what makes the Argentine economy run, complete with double-entry bookkeeping, stock fraud, tax evasion, embezzlement, phony expense accounts, dubious bankruptcy procedures, and the formation of dummy subsidiaries abroad.

In sum, Lewis believes that Perón, by following the fascist model of Germany and Italy, produced changes in the business class that "provoked their resistance, then their alienation and finally their withdrawal of capital. Faith in the future, once destroyed, is difficult to rebuild. That faith was lost during the watershed [early Peronism] and has not returned. That is perhaps the fundamental factor in the crisis of Argentine capitalism" (p. 243). Though Lewis's study is not a theoretical one, his identification of Peronism with fascism joins a continuing debate. Lewis argues that just because Peronism was popular with the masses doesn't exclude its proximity to fascism. After all, the Hitler and Mussolini regimes were more democratic than the regimes they replaced. "The idea that fascism is organized upper- or middle-class oppression of the workers is a gross oversimplification, at best" (p. 242). Apart from this dubious characterization of fascism, Lewis does not sufficiently distinguish Peronism from fascism. Perón stimulated the emergence of class conflict, though he sought to channel it. He avoided the use of racism as a key unifying element. Peronism did confront monopoly capital (part of Lewis's own thesis) in a way that fascism never did. Finally, Peronism went beyond fascist working-class rhetoric to build lasting working-class institutions and identifications that survived Perón and may even survive Peronism.

PETER RANIS

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Political Reform in Post-Mao China: Democracy and Bureaucracy in a Leninist State. By Barrett L. McCormick. Berkeley: University of California Press, 1990. 235p. $34.95.

Political Reform in Post-Mao China asks about the prospects for reforms aimed at rationalizing administration and creating a more autonomous society in the People's Republic of China (PRC). It concludes that political reform in Leninist states is bound to fall far short of the goals and it provides an institutional explanation, illustrated in three case studies of reform in the post-Mao period.

McCormick argues that Leninist state institutions generate a unique variant of Weberian *patrimonial rulership*, which produces many of the problems reformers aim to resolve and also inhibits implementation of reforms. Leninist state penetration of society is the major source of patrimonialism. Pervasive and monolithic formal organization in Leninist states gives officials personal discretion over a wide range of goods and services necessary in daily life. As a result, individuals in society pursue interests through informal particularistic relations with officials.

Patrimonialism affects reform in two major ways. First, it limits the state's technical capacity and alienates society. These problems motivate reformers to attempt changes but without challenging essential Leninist foundations such as the leading role of the Communist party. Second, state-generated patrimonialism inhibits reform. Reforms are implemented as campaigns, from the top down through the organizational hierarchy. One consequence is that the extensive networks of patron–client ties can blunt the ability of reformers at the top to implement policies. Reforms aimed at strengthening rational-legal authority are resisted by cadres at lower levels because such reforms mean less discretionary authority. Another consequence is that top–down reforms to strengthen rational-legal authority prove self-defeating: they reinforce the organizational monopoly that generated patrimonialism in the first place. In sum, Leninist institutions have created "a social and political logic" that requires change but also inhibits it.

McCormick presents three cases to illustrate the relationship between patrimonialism and reform in the post-Mao People's Republic of China: the campaign to strengthen socialist law, the 1980 elections to local people's congresses, and the 1983–87 party rectification.

McCormick's argument about the institutional sources of patrimonialism in Leninist states is not an unfamiliar one, nor is its specific application to the PRC. Andrew Walder coined the term *organized dependency* to summarize that relationship in his study of Chinese state industrial workers in *Communist Neo-Traditionalism* (1986). Jean Oi's study of clientelism in the Chinese countryside, *State and Peasant in Contemporary China* (1989), affirmed the scope of that relationship. McCormick's contribution is to consider the implications of organized dependency for political reform. While he sometimes overstates the usefulness of the Weberian framework to our understanding of Chinese politics, his thesis is a thoughtful addition to the literature on post-Mao reform, much of which is purely descriptive or explanatory in only idiosyncratic terms. McCormick's argument is strongly conceptual and framed in general terms. Since McCormick's question and explanation are generalizable, his study also contributes to the literature on change in socialist systems and is presented with appropriate background information in text and notes to make it accessible beyond specialists in Chinese politics.

McCormick makes good use of a large theoretical and empirical literature on clientelism and corruption in socialist systems and integrates relevant secondary sources into his analysis throughout the study. His use of primary sources in the three illustrative cases of post-Mao reform is sufficient to demonstrate his argument, but the empirical contribution does not really measure up to the theoretical one. In the past few years a number of studies have added to our understanding of Chinese politics by making use of new opportunities for formal and informal interviews with officials and ordinary citizens on and off the mainland as well as access to materials. Primary sources for McCormick's case studies are mostly newspaper and journal articles. The result is not fully satisfying by comparison. Nonetheless, McCormick's thesis in *Political Reform in Post-Mao China* clearly merits attention by specialists and in the field of comparative communism generally.

MELANIE MANION

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Solidarity and the Politics of Anti-Politics: Opposition and Reform in Poland since 1968. By David Ost. Philadelphia: Temple University Press, 1990. 279p. $34.95.

As political movements come and go, the Polish trade union known as Solidarity has experienced a sustained relevance to Polish society that is unique in the modern world. The strategy by which Solidarity survived continual repression is the subject of David Ost's book. He depicts Solidarity as possessed of a clearness of style and certainty of its goals that has allowed it to prevail despite conditions of martial law, loss of its legal charter, and continuous harassment up to 1989. That strategy, he has determined, is rooted in both a societal aversion to formal politics and a deliberate effort by the original core of Solidarity—especially the intellectual community—to ignore the state structure and focus instead on creating a new civil society.

The thesis of Ost's work is unique. Always representing a yawning gap between the state structure and the polity, postwar Polish politics has been an exercise in massive exclusion, followed by selective inclusion. Thus, intellectuals and reformers within the ranks of the Polish United Worker's Party (PUWP) were alternately sought after by the party hierarchy or expelled from their positions of input. Moreover, those who saw reform of the system as being possible continuously gave the party the benefit of the doubt, hoping that the next crisis encountered by the state would mean their reinstatement within decision-making circles. Finding their participation ultimately precluded, the opposition eventually turned to what Ost calls "the politics of anti-politics," that is, an abandonment of utopian dreams or revisionist politics in favor of organizing people through civic activities and thereby transcending the state. In this fashion the state could be considered irrelevant to the everyday lives of the Polish people.

The realization of this strategy came only in 1980 with the formation of Solidarity and the by-then-apparent bankruptcy of PUWP politics. From this point on individuals had a forum of sorts for involvement and a new belief to be accepted, namely, that people should act as if the system was already democratized (p. 68). Independent social activity therefore became the path to follow, regardless of governmental acquiescence. On the way toward this achievement, however, the essence of Solidarity itself was transformed. The trade union was both centralized—and at the same time decentralized—in its character. The boundaries between these two ideas "were purposely made unclear" by the Solidarity leadership so as to give the union greater flexibility in dealing with the state (p. 107). With the declaration of martial law in 1981, Solidarity found itself metamorphosing yet again, now as a conscious political force and one unable to perform its duties as a true trade union organization. While it would regain its status as representative of labor in 1989, Solidarity could no longer rescind its active involvement in the political process, especially after its sweeping victories in parliamentary elections in that year.

Ost defines the Solidarity period in terms of neocorporatism and acknowledges the work of Philippe Schmitter in this field. In neocorporatism Ost sees a logical response to state-dominated society and offers the observation that neocorporatism can be "as democratic as electoral pluralism," since it guarantees access to power for groups typically excluded by the regime but still in need of representation (p. 118). Solidarity's leadership, he contends, actively and deliberately pursued a neocorporatist approach without the knowledge of any other approach that might have been available for political action.

Ost's utilization of neocorporatism is not entirely satisfying. The approach does effectively assess the quandary of Polish politics: the party had coercion on its side but could not govern, while Solidarity maintained the allegiance of the masses but had no avenue through which it might channel its massive energies (p. 125). Throughout his work, Ost continually equates the leadership of Solidarity with the whole of the movement. Moreover, he sees neocorporatism as a conscious choice of a leadership in search of a guiding force or winning strategem. What is unconvincing in this argument is that Solidarity's leadership was never so cohesive and in many respects was quite divided (as evidenced by the underground period during martial law). Ost neglects the working-class character of Solidarity in favor of discussing its intellectual side. Almost nothing is said of those leaders arising from working-class origins (comments on Zbigniew Bujak are an exception) and how the average worker responded within Solidarity to the use of confrontationist policies. The importance of the working-class contribution to Ost's theory is seen in his own recognition that the strikes of 1988, which were largely uncontrolled by Solidarity, forced the government into a position whereby it needed to bargain to retain its political position (pp. 207–8). The resulting concession by the PUWP of a system of electoral participation demonstrates that the rank-and-file of Solidarity were not willing to settle for neocorporatism—a point that deserves the author's attention.

Overall, Ost's book represents a considerable contribution to both our knowledge of the events in Poland over the last decade and to our efforts at theory building for understanding nondemocratic societies. The book is a laudable effort in a field that typically produces overly subjective critiques rather than focused analysis. In this respect, there are few works with which it may be compared. It is a welcome discussion of political development.

MARK A. CICHOCK

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European Politics Reconsidered. By B. Guy Peters. New York: Holmes & Meier, 1991. 293p. $39.95 cloth, $19.95 paper.

Guy Peters has done what needed to be done. In a single volume, he has successfully managed to make sense of two decades of major political change in Europe. The task must certainly have been daunting. Volumes have been published on each of the themes he addresses. To treat them all in a way that is both succinct and clear is commendable. This book will become a seminal text and should be required reading for anyone remotely interested in current research on European politics. It could not have come at a better time. As the political map of Europe undergoes dramatic transformation, Guy Peters has provided a standard against which to measure future developments.

Chapters of the book cover major themes that have occupied the attention of scholars: the demise of the nation-state, postindustrial value change and new social movements, dealignment and realignment of political parties, corporatism, fiscal crisis, and the welfare state. Little eludes Peters's broad net, leaving the reader with a keen sense of the current European state of affairs. The first issue Peters tackles is the purported demise of the nation-state. Depending on where you trace its origins, the nation-state has served as the organizing principle of international relations for over two centuries. It is originally a European concept, transposed on the rest of the world with varying degrees of success. Peters points out how this concept now meets fundamental challenges in its own backyard. And the current challenges to European nation-states come from above as well as below. From above, the plodding yet determined integration represented by the European Community confronts national sovereignty. Also, technological society has produced a number of policy problems that do not recognize national boundaries (e.g., environmental degradation). From below, the notion of nationhood, so long a major concern of European leaders, seems destined to crumble despite the best efforts to overcome the parochial concerns of ethnic and regional groups. The attack on two fronts exposes the limitations of continued adherence to seventeenth-century concepts.

Economic and technological change has also altered the political culture of European democracies. The "acquisitive" politics of the postwar period have been superceded by the concerns of a generation raised and socialized in a condition of relative affluence. The new values seem to be a common feature of the West European countries and are producing relatively similar outcomes, such as concerns with new types of policy issues. Dissatisfaction with traditional modes of interest representation, particularly through the party systems, has given rise to new social movements and the small parties of the "libertarian-left." These changes have shaken the older political parties out of their snug and sometimes smug positions. In some countries the result is dealignment of the political system as citizens stop voting the way they used to or simply stay away from the voting booth. In other countries, realignments appear to have occurred, as permanent changes in voter preferences become apparent.

The changes that have transformed party systems have also shaken other political institutions. Perhaps the biggest of these is the challenge to the institutions of corporatism, long considered the typical policy style of most European countries. Corporatism appears to be at an end virtually everywhere, or at least under severe challenge. At the same time, the academic understanding of the phenomenon of corporatism is exceedingly inadequate. Peters examines the conceptual stretching that has characterized this body of literature. At the same time, however, he falls into the same morass most scholars of corporatism appear stuck in. The biggest problem with the current theorizing of corporatism is its lack of historical focus. The theories identify corporatism as the institutionalized representation of capital and labor interests in policy making. What they fail to recognize is that in most countries where corporatism is strongly entrenched, it originated from religious, particularly Catholic, political ideology. By reducing corporatist interests to economic ones, existing theory cannot explain corporatism in its historical context and overlooks substantial divisions within the labor movement in countries where one finds Catholic, as well as socialist, labor unions. Furthermore, if we accept the claim that European societies have entered a postindustrial age, economic groups are less important policy actors than professional groups. Indeed, labor unions in European countries seem to be focused more on professional than ideological themes these days. Such concerns are crucial to the further study of corporatism, and Peters does us a bit of a disservice by overlooking them.

In two separate chapters, Peters discusses the problems of governing amid fiscal crisis and the development of the welfare state. The order of the two chapters is perhaps backward. It makes more sense to talk about the welfare state before talking about fiscal overload, since much of the fiscal crunch has resulted from the rise of the welfare state. Be that as it may, the two chapters provide a marvelous overview of differences in social welfare programs across countries, problems of management and quality of services, and various efforts to cut back on expenditures. In summarizing the theme, Peters draws attention to the potential irrelevance of the welfare state in future. Social insurance programs are becoming meaningless for the rising number of unemployed, many of whom fail to accrue entitlements while they are out of work. In addition, there are indications that private sector insurance programs are gaining ground in Europe. While the private welfare sector has been the greater part—and the often-neglected component—of the U.S. welfare system, increasing reliance on the private sector in Europe could serve to undermine the solidaristic foundation on which the welfare state was built.

One of the most notable contributions of this work is its explicit inclusion of the smaller democracies of Europe. Unlike many other texts, Peters has not generalized his claims about the state of European politics from the experiences of the big four but also includes information on the smaller democracies in each chapter. There is one caveat in the scope of countries included in the book: it is about West Europe. Despite a cover photo of jubilant youth amassed atop the Berlin Wall and a title that suggests it is a book about the new Europe, Peters provides only a brief discussion of the revolutions in the East. Editorial scrutiny presents another problem in the book. While it includes a bibliography of twenty-some pages of every important piece of literature, much that is cited in the text does not appear in the bibliography.

As an overall assessment, this is an excellent book. Peters not only treats a great number of issues with care and skill, he does so with a broad perspective. This book is required reading for anyone with a scholarly interest in Europe. Even those who just teach a course on European politics will find the handling of many ideas clear and precise. Numerous tables full of basic but otherwise hard-to-find information will give extra detail to old notes. For class use, the book is appropriate for upper-division undergraduates and graduate students. Indeed, graduate students specializing in European politics will find the text full of suggestions for further research that could provide the themes for numerous dissertations.

ROBERT H. COX

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Right-Wing Military Government. By Robert Pinkney. Twayne's Themes in Right-Wing Politics and Ideology Series, no. 3. Boston: Twayne, 1990. 179p. $29.95 cloth, $14.95 paper.

In *Right-Wing Military Government,* Robert Pinkney endeavors to distinguish right-wing military governments from socialist and non-right-wing military governments. His work is divided into three parts. In the first part, he discusses conditions that precede military intervention in the political process, presents a description of soldiers "as Political Animals," and finally introduces the operational features of right-wing military governments. The second part consists of nine case studies divided into the four "variations on right-wing themes": personal rule (Chile), military authoritarianism (Uruguay and Pakistan), civil-military authoritarianism (Guatemala, El Salvador, and Bolivia), and limited democracy (Thailand, Indonesia, and Turkey). In the third part, Pinkney examines the politics of military disengagement and civilian succession.

Pinkney defines right-wing military intervention as "soldiers displacing democratically elected governments and either attempting to reverse previous left-wing policies or ensuring that previously threatened left-wing policies fail to materialize" (p. 2). In contrast, socialist military governments' "ideologies and stated policies are the mirror images of those of the right-wing regimes" (p. 15). The residual grouping of non-right-wing military rulers, such as Bokassa, Gadafi, Nasser, Ne Win and Rawlings, differs from right-wing governments because there is never an explicit commitment to the achievement of pluralist democracy as a "major objective" (p. 143). Pinkney's criteria for placing cases within the three categories of military governments need further elaboration.

Pinkney argues that right-wing military governments are characterized by six central and five "optional extra" features (p. 14). These attributes are apparently distilled from the central elements of conservatism, authoritarianism, and fascism. The most important characteristics are "a belief in the virtues of capitalism and private property, and indifference or hostility to social equality" (p. 13). It is clear that "the legacy of right-wing military government is . . . a largely negative one" (p. 124). According to the author, however, military withdrawal from government "might, given the right combination of luck and judgment [Machiavelli's *fortuna* and *virtu*] allow pluralist democracy to flourish" (p. 167). This is a simplistic and rather quixotic conclusion. The social processes underlying the emergence of democratic political systems amount to more than voluntarism imposed on material conditions. In the end, in Pinkney's view, it may be history itself that precipitates the arrival and eventual growth of pluralist politics: "Unlike our ancestors, . . . political actors in countries emerging from military government today know that pluralist democracy has worked in other parts of the world, and sometimes in their own countries, with varying success" (p. 155).

The author emphasizes the necessary elements for pluralist democracy in terms of minimum levels of basic indicators of development, such as per capita income, literacy, industrialization, and urbanization: "If a country is 'underdeveloped' in terms of the indicators we have used . . . it may be difficult to find acceptable civilian elites able to generate sufficient legitimacy from a largely apolitical population" (p. 133). Pinkney concludes that "socioeconomic development does not guarantee military disengagement, but the lack of development ensures that any military withdrawal will be only partial or temporary" (p. 135). Finally, he cautions against "purely 'political' explanations of the behavior of military governments" (p. 106).

Despite the frequent use of classificatory schemes and tables in an effort to clarify and summarize his views, Pinkney does not succeed in his goal of describing, explaining, and possibly predicting the presence or absence of right-wing military governments in politics. Indeed, one is left to wonder whether the author really has a knowledge of the military as an institution and as a political actor. Pinkney's understanding of military professionalism is that of an association or a guild, "like doctors . . . belonging to a distinctive hierarchy which has distinctive relations with its clients. The difficulty is that whereas the doctor's or lawyer's clients are easily identifiable, the army's client is the state on whose behalf it fights, and defining what is in the client's interests is a matter of political judgement" (p. 30).

Modern military forces neither have the state as a "client" nor determine its public policies. Rather, they are an instrument of the state, implementing its political determinations. Once the military does intervene in politics, it has ceased to be a professional force in terms of its subordination to civilians as well as its adherence to norms of authority and obedience. Any modern military institution seeking to govern (colonize the state) is doomed to fail because the political, being inherently uncertain, is intrinsically incompatible with an institution that as a part of its professional credo "does not deliberate," that is, engage in politics.

The processes of democratization and the consolidation of democracy witnessed during the last two decades throughout the world are of momentous significance. A careful review of the book's bibliography and the author's frequent and repetitive citation of single contributions to anthologies, reveal a limited use of valuable resources widely available to the student of civilian-military relations. Innovative contributions to the study of political processes in the world require more than a selective study of countries with diverse backgrounds whose "experience of right-wing military government gives them common features and problems" (p. 159). It is unfortunate that what most impresses the reader of this volume is the synthesizing ability of the author, along with the apparently important role played by serendipity as a criterion for the selection of the literature used, as well as for the cases chosen for study.

MICHAEL GOLD

American University

The Political Economy of Mexican Oil. By Laura Randall. New York: Praeger, 1989. 228p. $45.00.

Political economists come from several traditions. Some are political scientists who examine linkages between politics and economics through one or another systemic perspective heavily influenced by realist ideology. Their analyses feature conflicts between and among states, firms, and social groups that take the form of epic struggles over wealth and power. Others are rooted in the tradition of neoclassical economics. Its dominant ideology is cooperative rather than conflictual: states and firms struggle alongside one another or even in concert against such enemies as poverty, inefficiency, and personal corruption, as often or even more than they struggle against one another. Laura Randall belongs to the latter group.

Randall's book about PEMEX, the Mexican national oil company, is similar in tone, as well as in style, to Louis Turner's *Oil Companies in the International System* (1978) and Edith Penrose's *Large International Firm in Developing Countries* (1968). All three authors provide richly empirical and highly evolutionary pictures of the political and economic environment in which the firms they examine operate. The greatest virtue of these books—and this is certainly true of Randall's—is the immense amount of factual information they contain. Randall is thorough in her discussions of the employee benefits supplied by PEMEX, pricing, efficiency, the impact of PEMEX on the Mexican capital goods sector, and the general conditions of labor in the industry and in the country as a whole—the subjects of the four strongest chapters in the book. She provides detail, connections between PEMEX and other economic actors in the domestic and international environment, and general observations that can be applied to other firms in other countries. For example, she discusses the gradual convergence in wages between employees of PEMEX and those of other firms and state agencies in the context of the structure of labor markets in developing countries generally. At first, oil industry wages are higher because of the wealth of the industry (its capacity to bid high for the best workers is usually greater than the capacity of other agencies and firms) and the lack of skilled native workers, which pushes their price up. As oil money cycles through the country's educational system and as the firm itself trains workers, the pool of skilled labor increases. Because of the larger supply of skilled workers and because more native skilled workers are employed by other firms, wage differentials shrink.

The perversities of oil-based economies are also visible in Randall's study. She traces the high rates of air pollution in Mexico City and the choice of an energy-intensive (rather than a labor-intensive development strategy) to the political choice of a cheap oil policy. The level of consumer prices for oil products is itself a function of political decisions. Some are contained in national development plans, which are the product of the interaction between government officials and interest groups, while others are the result of decisions made by national price boards and are influenced most strongly by the government's fiscal position and its ability to borrow abroad (p. 51).

What I don't find in this study is an examination of the multiple—and, I assume, at least occasionally conflicting—interests among the actors concerned with Mexican oil policy and the role of PEMEX in its generation and implementation. We read of possible corruption during the regime of PEMEX director Jorge Diaz Serrano and of the disagreement over his essentially unilateral price cuts in 1981; but the structure of the interests of the various sides in these stories is unexplored. What are the interest groups with the power to affect national economic plans, and how is this influence brought to bear? Which, if any, must be placated in other ways when fiscal duress reaches levels sufficient to produce product price increases anyway? Is there a connection among the Partido Revolucionario Institucional, Mexico's ruling party since the revolution; PEMEX; and the content of oil policy, such as decisions about the distribution of production and its related economic activities within the country? And even though the wages of PEMEX workers are converging with wages throughout the economy, those wonderful benefits remain unique to PEMEX. Where did they come from? How have they been protected, despite their high cost, during the economic crises that have afflicted Mexico in the 1980s?

It may be that the conflicts in Mexican oil policy are connected less to structural relationships in the political economy as a whole and more to personalism and exogenous events than they are in the United States or many countries of the Middle East. Even if this is the case, it would still be instructive to explore these issues explicitly and describe how this unusually high level of consensus was achieved and is maintained. If such a study had no other effect, at least it would shake the bias toward the analysis of conflict.

The Political Economy of Mexican Oil provides readers with a wealth of information about PEMEX and its activities. It has also made me want to understand better the very different worldviews that political economists bring to bear on their various analyses and the effects these worldviews may have on their conclusions and the advice they offer to policy makers. On the one hand, concentrating on conflicts could actually make them worse by providing rationalizations that can be used in personal and institutional rivalries whose role would be relatively minor in the absence of an independently valorized intellectual framework within which to carry them out. On the other hand, ignoring or minimizing the structures of interests make it difficult to get out of oppressive situations because their root causes are so obscure—and obscured. It is possible that the power of ideology and disciplinary training is so constraining that these two kinds of political economists would find it difficult to communicate, much less to collaborate. However, I think it would be very worthwhile to try to do both.

MARY ANN TETREAULT

Old Dominion University

The Israeli Arms Industry: Foreign Policy, Arms Transfers, and Military Doctrine of a Small State. By Stewart Reiser. New York: Holmes & Meier, 1989. 252p. $55.00.

This specialized and detailed study of the Israeli arms industry in development from prestatehood down to the present is a significant work on an important subject, albeit one in which the reader must frequently labor through an abundance of detail to sometimes discern important developments and concepts.

The book includes a wide range of cited source material and references, and in outline and chapter arrangement has a logical progression of topics laid out in historical time periods. Reiser gives a thorough account of the development of the Israeli arms industry from very modest beginnings down to its very sophisticated contemporary stage, where it occupies an important position in the world concerning weapon systems development, manufacture, and arms transfers. Arms exports provide Israel with over one billion dollars a year in foreign currency, and almost 30% of the work force of industry is employed in the arms field.

The input of various Israeli figures in politics, science, and industry is traced for the various periods of development that are set within political and foreign policy events and evolution. Difficult decisions for Israeli planners were constantly present concerning weapon systems development concerning various pros and cons involved. The Lavi plane project is a particularly important example, and Reiser does an excellent job in going through that subject with its many complications and ramifications to its final scrapping. The need to expand foreign sales sometimes had the potential of jeopardizing Israel's security; indeed, in December 1986 Defense Minister Yitzhak Rabin informed the Knesset that Israel exported newly developed military equipment against the opinion of all the field security agencies that technological secrets could be revealed by such sales.

The Israeli defense industry and its activities have reflected both strategic concerns of defense, and commercial aspects on the international market. The author delineates how Israel in the aftermath of the 1973 war decided to try to fill every strategic niche for itself. At the same time, however, an increased involvement with the United States is traced (a U.S. 37% funding for the Israeli defense budget in 1982 and now heading toward half). Arms sales to Central America and the Nicaragua Contra ties are set out, as well as to the Islamic Republic of Iran. Reiser emphasizes not only the great importance of Israeli weapons production and sales-transfers in foreign policy but additionally asserts that the country's military industries have come to dominate the economy of the state (the average growth rate annually from 1967 to the mid 1980s has been approximately 20%-24% per year). The author contends that economic considerations seem to have assumed primacy for Israel's energetic arms exports operations since 1973 although strategic and political factors remain important.

An important point that Reiser advances is the development of institutional alliances between sectors of the establishment, including personal ties between some company directors and officials of the Defense Ministry that he characterizes as almost incestuous in nature; the Arava STOL aircraft project was initiated despite an international market research survey that was negative and despite the insistence of the Israeli Air Force that it neither wanted nor needed the plane.

The Israeli Arms Industry is probably not for general readers but for those who are particularly interested in arms industry development and arms transfers and in Middle East studies relating to military affairs and foreign policy implications. It is a worthwhile and leading study in the field.

SCOTT D. JOHNSTON

Hamline University

Minority Government and Majority Rule. By Kaare Strom. Cambridge: Cambridge University Press, 1990. 293p. $39.50.

Adding to the growing literature on political democracies, this book is concerned with the question of minority government formation and performance in parliamentary systems of government. The author holds, quite rightly, that these questions are worth addressing for two fundamental reasons. First, minority governments are not as infrequent a phenomenon as many might think; earlier studies concur in putting their number at more than 30% of postwar parliamentary governments. Second, conventional explanations of minority governments generally depict them as pathological; they are commonly seen as being the product of unhealthy democratic conditions like political crisis and instability or party system fractionalization and polarization and as performing less well in office than majority governments. But if minority governments are pathological, the author asks, why do they form so often, and why have there been so few crises of democracy in postwar Europe? These questions are the starting point for the author's efforts to relate minority government formation to "rational actions by the political parties involved" (p. 16).

Using a rational choice framework, Strom explicates and puts to the empirical test a probabilistic theory of minority government formation. His starting point is the rejection of the commonly held view that membership in a majority government is the prerequisite to achieving the political power and policy influence sought by party leaders. Depending on the degree to which parliamentary power is centralized, both goals can also be realized by being a minority government or by being in opposition. Thus, minority governments result when core members of the coalition choose not to seek a majority government and/or when marginal parties choose not to join the coalition. In both cases, the costs of majority government participation are deemed by the political party leaders concerned to outweigh the benefits. Benefits are argued to be defined by party leaders in terms of policy influence and costs in terms of the impact of governmental participation on both future electoral performance and coalitional bargaining power after the next election: "Minority governments should be most likely to form when the benefits (of majority government participation) are low and the costs great" (p. 49).

The bulk of the book (four of six chapters excluding the conclusion) is given over to testing this thesis. The methodology is laudably eclectic. Chapters 3 and 4 presents findings from a cross-national aggregate data analysis that broadly support Strom's expectations of minority government formation (chap. 3) and performance (chap. 4). The first of them further suggests that future electoral success is more important than immediate policy influence in the decision whether to participate in government or not. With regard to performance, chapter 4 indicates that minority governments generally do less well than majority governments but do better than majority coalitions for policy influence and electoral advantage. Strom's rational choice explanation is then further validated by in-depth case studies that demonstrate its applicability in two countries, Italy and Norway, that differ markedly in terms of the contextual factors that might be expected to influence minority government formation and performance.

The argument presented in this book is straightforward and convincing; minority governments form largely because political parties do not always perceive it to be in their future electoral interest to enter majority coalitions. Moreover, minority governments do not perform systematically more poorly than majority ones. The testing of this argument, however, is relatively complex and affords plenty of opportunity to quibble with details of operationalization and interpretation. It is also difficult to keep track of at times, a problem that the author attempts to overcome by means of frequent summaries and recapitulations. But such problems should not overshadow the book's strengths. In forcing a new look at minority governments, it undermines the validity of the majoritarian assumption on which much of the coalition theory in the rational choice tradition is predicated. It forces us to think afresh about the relationship between types of party system and patterns of government formation, and it should stimulate interest in an important and little-researched area of the democratic political process. Overall, it is a fine example, in both theoretical and operational terms, of cross-national comparative research.

ANTHONY MUGHAN

Ohio State University

More Power to the Soviets: The Democratic Revolution in the USSR. By Michael E. Urban. Brookfield, VT: Edward Elgar, 1990. 167p. $37.95.

For those interested in the Soviet Union, *More Power to the Soviets* provides a fascinating and well-documented analysis of the democratic revolution in the Soviet Union, beginning with Gorbachev's crucial speech on democratization to the Central Committee Plenum in January 1987 and ending in mid-1989 with the completion of the first session of the newly empowered Supreme Soviet. But this book is not of interest only to Soviet specialists. It is a well-crafted and theoretically informed case study that would be instructive to those concerned with such issues as the process of political liberalization, the origins of social movements, the complex relationship between reform from above and reform from below, and, finally, the many contradictions involved when states seek both to do things in a new and more efficient way and to hold on to their old habits, interests, and bases of political power.

Michael Urban presents three main arguments. First, Gorbachev needed to liberalize Soviet politics if he was to succeed in restructuring the Soviet system. Gorbachev, therefore, is best understood as a liberalizer, not a democratizer, and as a centrist situated between the reform elements (primarily outside the party-state apparatus) who wanted liberal democracy and those (primarily inside the party-state apparatus) who wanted a more efficient and effective dictatorship.

Second, Urban argues that the party was relatively successful both in diluting the liberal thrust of the proposals for reform *and* in minimizing the costs to them of a more liberalized political environment. For example, the electoral reforms enacted at the end of 1988 allowed for political competition but also enabled the party to function as the majority (albeit facing a vigorous opposition) in the newly constituted Supreme Soviet. This was because elections to the Supreme Soviet were indirect; because one-third of those electing the Supreme Soviet (i.e., the members of the Congress of People's Deputies) were representatives chosen not by popular election but by major organizations and institutions; and because the party could control (in many, but not all cases) the selection of candidates vying for the seats in the Congress of People's Deputies that were chosen by popular vote (two-thirds of the chamber). Moreover, the party managed to limit the powers of the newly structured legislature, to leave presidential powers loosely defined, to slow the pace of judicial reforms, and to put off the question of renegotiating the Soviet federal system.

Third, the reforms nonetheless went very far in liberalizing the Soviet political system. In many ways, this is Urban's central point. The Soviet system managed to go so far down the liberalization path from 1987 to 1989 primarily because the reform from above stimulated—primarily by accident—significant pressures for democratization from below. Why this occurred had to do, first, with changes in the political discourse. By altering the political discourse, Gorbachev allowed for—indeed, legitimated—a new discourse that was far more critical and far more open to alternative visions of the future than had previously been the case. This discourse, moreover, took on two dimensions: a public debate that contained some new ideas but was orchestrated from above and a private debate that by the fall of 1988 produced (particularly in urban areas and in the western portions of the Soviet Union) a "celebration of democracy" (p. 81).

At the same time, the space for popular mobilization in support of pluralist politics expanded considerably. This was in response to a variety of factors: the window of opportunity provided by a party entertaining liberalizing reforms; citizens gaining the experience of participating in the nomination of candidates and competitive electoral campaigns (two-candidate elections were in fact the norm for the two-thirds of the seats elected by the public in the March 1989 elections); liberalization of the print and broadcast media; the formation of popular fronts in support of perestroika beginning in the spring of 1988; the many and increasingly visible shenanigans of a party eager to claim at least an oligopolistic political position; and the willingness and the capacity of "deviants" within the Supreme Soviet to take advantage of various loopholes by, for instance, forming an opposition coalition (led, in part, by the well-organized representatives from the Baltic states) and by utilizing to the fullest the rights of that chamber to subject nominees for government posts to considerable cross-examination.

These three arguments lead Urban to conclude (as the title of his book suggests) that the changes in the Soviet Union have been revolutionary in the sense of making a sharp break with the past but at the same time have fallen far short of a democratic outcome in which "all" power would be transferred to the legislative system. Thus, politics in the Soviet Union has been deregulated. The party is not without self-interest or significant powers, the expansion of civil liberties is still provisional, democratization from below could very well intersect with competing definitions of national and state boundaries (as it has of late), civil society is only beginning to form, and the democratic opposition will be easily divided, once it moves beyond the issue of simply saying *no* to the apparatus. But—and this is a big *but*—the house built by Stalin is not likely to be resurrected.

VALERIE BUNCE

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Between Profit and State: Intermediate Organizations in Britain and the United States. By Alan Ware. Princeton: Princeton University Press, 1989. 308p. $42.00.

In the pluralists' analyses of the United States and British political systems, considerable emphasis has been placed on a "third sector"—organizations that fall outside the business sector on one side and the government sector on the other. They are what Alan Ware calls "intermediate organizations," such as charities, churches, nonstate universities, consumer cooperatives, credit unions, and social clubs. Pluralists read virtues to be prized in these intermediate organizations: their independence of both government control and business profit seeking, their ability to secure the participation of armies of volunteers, and their contribution to the amelioration of social problems. But, Ware argues, in each of these respects pluralists have seriously overstated the contribution of intermediate organizations.

First, they are not really independent of the other sectors. Their legal definition is such that they may not *distribute* profits; but that is not to say that they never *make* profits. Ware cites the increasingly entrepreneurial activities of museums, university presses, and, in the United States, university athletic departments. In many cases, such as consumer cooperatives, intermediate organizations are in direct competition with private businesses. And with respect to building societies in the United Kingdom and savings and loan associations in the United States, the original purpose of creating institutions not driven primarily by competitive market considerations has long been forgotten. (Strangely for a book published in 1989, Ware's discussion of the savings and loan associations makes no mention of the financial cataclysm that had been the subject of angry debate for some time.) At the same time, the heavy reliance of many charities, hospitals, and "private" universities on various forms of government funding raises serious questions about the degree of autonomy from the state.

As for the contribution of volunteers, Ware recognizes their potential value to the functioning of democracy but argues that in many organizations volunteers are used largely for menial tasks and have little say in policy making. Nor is Ware impressed by the contribution intermediate organizations make to social problems. The grants made by even the biggest foundations pale into insignificance compared with the billions provided by government. And when the Reagan and Thatcher governments cut government support for poverty programs and the arts, private contributions were bound to fall far short of filling the gap (especially in Britain, where there is a much weaker tradition of giving to charitable causes than in the United States). Further, Ware sees the part played by intermediate organizations as interest groups in the public policy arena as having only a limited impact.

Perhaps—Ware suggests in a postscript, "Pluralism and the Future of Social Democracy"—intermediate organizations might perform better in a social democratic system; and he recommends that democratic socialists, who have lost some of their commitment to central government, look again at the socialist pluralism of G. D. H. Cole and the early Harold Laski as well as the later Robert Dahl. But for intermediate organizations' contribution to democracy in capitalist societies Ware is only able to offer one, rather faint cheer.

Others will undoubtedly disagree with the extent to which Ware has diminished the present importance and value of intermediate organizations. Foundation grants may not match the scale of government subsidies, but they have led the way in fields where government would not move. The organized pressure of intermediate organizations has surely accomplished more in such areas as environmental and consumer protection than Ware allows. Cultural and charitable organizations may sell products at a profit and receive government grants and still be quite different from business or government.

Moreover, it would be too much to say that Ware has succeeded in his declared aim of remedying the absence of any "satisfactory analytic frameworks in political theory into which the now-burgeoning empirical studies could be fitted" (p. xi). He has clearly weakened one aspect of the traditional pluralist case. But he has not provided a satisfactory analytic scheme within which the incredibly diverse range of institutions he discusses can be encompassed. For example, trade unions are nongovernmental and nonprofit organizations. But it is not at all clear from Ware's review of them at various points in his book whether or not they can be brought within the same general category as hospitals, museums, and universities.

Nonetheless, Ware has built an impressive argument with great thoroughness. Compressed into 259 pages of text, his book deals with an extraordinary array of organizations and provides the historical context and contemporary debate on issues ranging from fundraising methods, to the growth and transformation of volunteering, to government regulation of intermediate organizations. The book will be a useful reference for any future discussion and research on this topic.

LEONARD FREEDMAN

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Democracy in Black Africa: Survival and Revival. By John A. Wiseman. New York: Paragon House, 1990. 228p. $22.95.

In the face of independent Africa's propensity for authoritarian rule in the form of single-party regimes or military governments, can it be said that democracy is irrelevant for that continent? To this question John Wiseman answers with an emphatic *no:* "The major argument of [this] book is that democracy has played, is playing, and is likely to continue to play a significant role as a viable alternative to Third World authoritarianism" in Africa as elsewhere (p. 12).

Democracy for Wiseman means essentially representative democracy with free elections based on a universal franchise in which there is real competition and choice for the voters. Single-party regimes are thus not democratic in terms of his criteria. Employing this definition, Wiseman argues that since 1970 (about a decade after the beginning of the transferrence of European rule to democratically elected African governments) 18, or about one-third of African states, have either maintained or experienced some period of multiparty democracy.

Most of Wiseman's study is a discussion of the four states that have had uninterrupted democracy (Botswana, the Gambia, the Indian Ocean island nation of Mauritius, and Zimbabwe) and nine others that have revived it, however briefly—in order to provide an "explanation" for democracy's survival in an admittedly hostile environment. His central conclusion is that democracy's survival is not due to social or economical factors (except for the total economic collapse of a country) but rather to political ones. Of these, the most important is that authoritarian rule has simply not been able to deliver the goods, contrary to the arguments of those who rationalized it when it began to make its appearance in the postcolonial era. While the future is not certain, Wiseman argues that since Africa has now had ample opportunity to experience the dismal reality of authoritarian rule, it becomes increasingly likely that the democratic option will get more of a hearing and thus be realized.

While Wiseman correctly notes that his study "has no pretensions to make any significant contribution to democratic theory" (p. 3), it is unfortunate that he does not address, or even show awareness of, what the theorist C. B. Macpherson calls the "limits and possibilities of liberal democracy," which is essentially the kind of democracy he is examining. Thus, because of his framework, Wiseman misses an excellent opportunity to discuss the regime of Thomas Sankara the military leader (apparently authoritarian in terms of Wiseman's criteria); and discussion of the country ends when a mmilitary coup brings Sankara to power. Yet most impartial observers of the Sankara period agree that this was perhaps the only time in the country's history when there was meaningful mass involvement in the decision-making processes that resulted in real material gains for the majority.

Wiseman's data make clear that his analysis would benefit from a more comparative and theoretical approach. Of the four countries where democracy has been continuous, only one, Mauritius, has experienced the displacement of one ruling party by another via elections. In addition, the "Mauritian economic system is more developed along capitalist lines . . . than almost any other in Africa" (p. 71). Last, democracy has survived in Mauritius, as well as in the other three countries, because "political elites have made the system work" (p. 73).

The history of liberal democracy reveals that only after an extended historical period were the elites or bourgeoisie willing to allow for a relatively competitive political process, that is, only when it was confident that its class interests would not be threatened by a defeat in the electoral arena. In the Third World, the position of the bourgeoisie is much more precarious than its counterparts in advanced capitalist societies. (This might explain why in Latin America, contrary to Wiseman's expectations, authoritarian rule has survived for so long in spite of its failure to deliver the goods.) Furthermore, production suggests that Third World elites are not likely ever to achieve the level of security of their Western and Japanese counterparts. This could, therefore, explain why Mauritius is the exception that proves the rule, namely, that liberal democracy's prospects in Africa and the rest of the Third World are not very bright. Including Samuel Doe's Liberia and Milton Obote's second coming in Uganda as examples of democratic rule indicates what Wiseman must resort to in order to conclude otherwise.

In spite of its limitations, Wiseman's book is a timely addition to a growing body of literature on the debate about democracy in Africa. Although written before the crisis of Stalinism had completely unfolded, his overall claim, namely, that the democratic impulse is alive and well in Africa, is validated by the growing critique of single-party states throughout the continent in response to the events in Eastern Europe. At the same time, his analysis, because of its liberal democratic framework, is inadequate in explaining how that impulse will be actualized.

AUGUST H. NIMTZ

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INTERNATIONAL RELATIONS

After Reagan: Confronting the Changed World Economy. By C. Michael Aho and Marc Levinson. New York: Council on Foreign Relations, 1988. 245p. $19.95.

Beyond Globalism: Remaking American Foreign Economic Policy. By Raymond Vernon and Debora Spar. New York: Free Press, 1989. 246p. $22.95.

By now it has become commonplace to assert that U.S. foreign economic policy makers must operate in an increasingly interdependent global economy in which the United States is no longer the dominant (or "hegemonic") power. In different ways these books respond to the main features of the new international economic reality. The Aho and Levinson volume is the product of deliberations during 1988 of a study group on the "economic choices confronting the next president," convened under the auspices of the Council on Foreign Relations. The other study is coauthored by Raymond Vernon, who has commented astutely on issues of international economic policy for many years, and Debora Spar, a graduate student at Harvard.

The Aho and Levinson study is a wide-ranging assessment of virtually the entire panoply of international economic issues, including macroeconomic imbalances (especially those of the United States), exchange rates and monetary policies, debt, trade, aid, and foreign direct investment (especially by foreigners in the United States). Each chapter contains a cogent statement of the problems, data to illustrate their various dimensions, and—the primary emphasis of the volume—recommendations for dealing with them.

The result is what we have come to expect of the Council on Foreign Relations—a clear statement of "mainstream," "responsible," or "pragmatic" foreign policy options. At one level, the recommendations are so lucidly presented as to seem unarguable. Thus, the authors stress the primordial importance of reducing the U.S. budget deficit and offer a clearly reasoned plan for doing so over a five-year period. They argue the pros and cons (mainly the former, in their view) of the system of floating exchange rates and reject proposals to tighten international monetary cooperation in favor of the looser notion of "target zones" for the principal currencies. They repeat virtually all elements of the conventional wisdom concerning the international debt crisis, including the necessity of pursuing a solution on a case-by-case, voluntary, and market-oriented basis. They recite the usual case against protectionism in international trade and stress the importance of multilateral trade negotiations. They propose ways of dealing with the "sensitive sectors"—agriculture, steel, textiles, and "high-tech."

While the analyses of these topics are skillfully undertaken, the resultant recommendations are not particularly innovative or exciting. For example, more radical approaches to reducing the budget deficit (through substantial tax increases or deep cuts in military expenditures, for example) are nowhere to be found; consideration of an "international debt facility" or other mechanisms for substantially reducing the external indebtedness of developing countries do not merit attention; and there are no bold institutional proposals for reforming the International Monetary Fund, the World Bank, or other international institutions. However, precisely because the policy recommendations are essentially centrist and incrementalist, they seem likely to fall on welcome ears in the Bush administration. They appear eminently congruent with the president's predilections, both of tone and substance.

In a few areas, however, the tone is a bit more assertive and aggressive. Such is the case with the authors' vigorous critique of efforts to curtail foreign direct investment in the United States. They present an excellent assessment of the benefits likely to accrue from such investment and nicely contrast U.S. fears of foreign takeovers with our usual blithe assumptions about the benefits allegedly accruing from U.S. direct investments abroad. A similarly compelling argument is presented for increased serious attention to "adjustment assistance" programs for U.S. workers displaced by imports from abroad. The authors go beyond usually routine assessments of this topic, as they do in their discussion of the need for international harmonization of regulations regarding banking and securities markets.

Still, one who is searching for a reasoned statement of U.S. "establishment" thought on current issues of foreign economic policy could scarcely do better than to peruse carefully *After Reagan.*

Beyond Globalism has a range of topics only slightly less comprehensive than those addressed in the Aho and Levinson volume. Here, too, are chapters on the politics of international trade, foreign exchange, multinational enterprise, and foreign aid. But the emphasis is less on specific policy recommendations than on arguing the case for a particular U.S. approach to these and related international questions. The authors trace the remarkable achievements of U.S. foreign economic policy in the years immediately following World War II, including the familiar stories of the creation of the Bretton Woods institutions and the Marshall Plan. But the authors demonstrate that such achievements were essentially the products of very unusual, unlikely-to-be-repeated circumstances. It is not that the United States can no longer achieve anything in international economic relations, but more recent achievements—such as increasing the safety of international banks, improving international consultations over issues of monetary policy, or improving conditions for the international sale of services—seem decidedly more mundane.

Vernon and Spar spell out a number of reasons why the United States seems to find pursuit of its priority international economic goal—to maintain a "liberal" international economy in which markets flourish—increasingly difficult. (Of course, whether this in fact is the abiding U.S. foreign economic policy goal may be thought open to considerable question. Departures from this enshrined norm seem so often and important as to call into question such an overarching generalization.) Be this as it may, the reasons adduced for increasing difficulties are thoroughly presented and nicely argued. Structural features of the U.S. system, most notably the system of checks and balances and the marked diffusion of power, are primary explanatory variables. But other important factors are the values, perspectives, and strategies of other actors in the international system; and the authors offer capsule summaries of European, Japanese, and developing-country orientations. While they paint with an exceedingly broad brush, their generalizations seem accurate for the most part and tend to show the atypicality (rather than generality) of U.S. approaches. Thus, the United States is increasingly thrust into the vortex of international economic relations with some apparent handicaps.

While it is sometimes difficult to disentangle the argument, the gist of it seems to be the need to go "beyond globalism." The authors conclude that under present circumstances, global organizations operate under severe limitations in addressing the problems associated with maintaining open markets. Neither the U.S. government nor other governments desire to vest such power in global organizations, with their heterogenous mix of countries covering different interests and ideologies. However, the need for cooperation in international economic affairs continues to grow. Cooperation will emerge from smaller groups of countries, joined together by a common sense of promise or threat. The authors suggest that this is therefore a time for exploring a new terrain and for groping toward new patterns of cooperation.

What all this adds up to is not abundantly apparent, at least not to me. There is much talk of "a movement to smaller groups," "linked negotiations," and extending the "fast-track" approach to executive-legislative (and interest group) relations in trade and other negotiations. While some of the points are well taken, especially the discussions of how to expedite executive-legislative understandings on key issues of foreign economic policy, they do not appear to add up to a comprehensive or consistent approach to the dilemmas confronting U.S. policy makers in an increasingly interdependent global economy. The authors' "retreat from globalism" may strike some as eminently pragmatic and sensible; and so it may well be. But it leaves some unease in the face of global problems—international environmental issues being only the latest example—that do seem to require global solutions. While such solutions are admittedly espoused with all kinds of cheap rhetoric by the international civil service, there is more than a kernel of truth in arguments on behalf of broader—not narrower—approaches and policies.

ROBERT AYRES

World Bank

Bases Abroad: The Global Foreign Military Presence. By Robert E. Harkavy. New York: Oxford University Press, 1989. 389p. $74.00.

This volume provides an excellent summary of the problems and status of various overseas military activities, defined broadly as foreign military presence. The author focuses not only upon naval, air, ground, and missile bases abroad but also upon intelligence, space, communication, research, and environmental facilities.

Among the many strengths of the study is a general historical overview of the different kinds of basing activities, as well as how changing technologies, power relationships, and international conflicts have affected those activities. There is a particularly good discussion of U.S. intelligence collection procedures and how such capabilities are enhanced by overseas facilities.

Providing a general "snapshot" of global foreign military presence, the book includes considerable information that is dated, since it concentrates primarily upon U.S. and Soviet foreign military facilities abroad. Recent events such as the decline of cold war politics, the collapse of the Warsaw Pact, and the signing of various arms control agreements, have made the long lists of weapons and troops at various bases in Europe obsolete. Events in the Persian Gulf, on the other hand, remind us of the salience of overseas bases in the Third World. The author's discussion of these facilities, though not as extensive as those related to Europe, is likely to remain of interest. Given the long lead time required for getting troops and materiel to a foreign location, incentives will remain high for establishing and maintaining bases abroad.

As the author points out, several factors have tended to minimize the need for overseas naval bases including the greater range of modern ships, advances in refueling at sea, and smaller navies. This has allowed the United States to support a global naval presence with a handful of operating bases. Much the same may be said with regard to aircraft.

Of interest is the author's discussion of the role of overseas bases in coercive diplomacy with case experiences of the United States, the Soviet Union, and various European countries. Attention is also given to the importance of a foreign military presence in terms of arms control verification and environmental monitoring.

Whereas the book is primarily descriptive and likely to overwhelm the general reader with its sheer volume of information, the nonspecialist is likely to find the final chapter the most useful. In it Harkavy seeks to answer questions concerning the role played by formal alliances, arms transfers, and foreign aid in gaining access to foreign bases. The categorical analysis used for testing the relationship between these factors and basing policy is extremely rudimentary, and the author appropriately warns that causal explanations would require more in-depth case analysis. Nevertheless, some interesting patterns emerge. Large naval and air bases were located in countries that received major arms transfers from the United States, the Soviet Union, France, and (to a lesser extent) Britain. Foreign aid appeared to be an important ingredient in the willingness of states like Greece, the Philippines, Portugal, Spain, and Turkey to provide basing rights.

Concerns such as receiving protection against aggression as well as *quid pro quos* such as receiving rents for bases (or even trade preferences) are also discussed. A final section also examines efforts to prevent other states from offering basing facilities to a given government's adversaries.

LLOYD JENSEN

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Book Reviews: International Relations

The Soviet Union and the Politics of Nuclear Weapons in Europe, 1969-1987. By Jonathan Haslam. Ithaca: Cornell University Press, 1990. 227p. $34.95 cloth, $13.95 paper.

The German Nuclear Dilemma. By Jeffrey Boutwell. Ithaca: Cornell University Press, 1990. 247p. $29.95.

Enhancing European Security: Living in a Less Nuclear World. By Ian M. Cuthbertson and David Robertson. New York: St. Martin's, 1990. 223p. $49.95 cloth, $16.95 paper.

Perhaps inevitably, books dealing with U.S.-Soviet relations and the military-strategic issues of the mid- and late 1980s (prior to the revolutions of 1989-90) have a somewhat dated quality. The cold war has given way to an astonishing degree of U.S.-Soviet cooperation; both East and West Europeans appear determined to overcome the postwar division of their continent in an era of postcontainment; security issues that only a few years ago threatened to split NATO have lost urgency; and the fall of the Berlin Wall and the subsequent unification of Germany have fundamentally reshaped the European political order.

But this dated quality, which stems from the authors' insufficient foresight in not anticipating future events (and who would wish to assign blame for that?), also reveals and highlights the strengths and weaknesses in the conception of their analytical framework. The shakier the architecture of that framework and the thrust of the argument that proceeds within it, the more susceptible the analysis becomes to the dislocations that unanticipated events have inflicted on it. To have asked the right questions of the past, not to have provided the right answers for the future, becomes the essential criterion for analytical insight and political salience.

In the three books under review—all published in 1990—the authors escape the danger of working close to current events to a remarkable degree, primarily because their attention is focused (more or less) on the political background and the military-strategic implications of the December 1987 U.S.-Soviet accord, in which both parties pledged to eliminate their intermediate-range missiles and launchers in Europe. Since NATO's "double-track" decision of December 1979 to redress what is called the *Eurostrategic* nuclear imbalance (either through negotiated Soviet reductions or additional Western deployment) was as much an issue of alliance management as of U.S.-Soviet relations, its analysis yields important insight into the condition of the transatlantic security compact (particularly its U.S.-German backbone) and the political role of arms control.

Jonathan Haslam's book, which covers the Western as well as the Soviet dimension of the Eurostrategic issue as it emerged in the late 1960s, interrogates this complex material with exemplary precision and clarity. This is a lean book, focused and disciplined, in which Haslam spells out the conflicting interests of the United States, the Soviet Union, and the West European NATO members that led to the Intermediate-range Nuclear Forces (INF) controversy. The book elucidates the United States' endeavor to obtain "forward bases" in Europe and elsewhere and to implement its perennial geostrategic interest of emplacing its military power as far away from its own territory and as close to a potential adversary as possible. Equally plausible was the Soviet strategic interest in ridding Europe of U.S. forward-based systems or, when that proved impossible through negotiations (as in SALT I and II), to seek balancing compensation in creating its own extensive intermediate-range ballistic arsenal, primarily through the accelerating deployment in the 1970s of SS-20s. The West European position, complicated by the differing security problems and the differing nuclear status of the United Kingdom, France, and the Federal Republic, basically viewed U.S. forward-based nuclear capabilities as the symbol and reality of the U.S. nuclear guarantee to Europe, especially after the advent of U.S.-Soviet parity on the level of intercontinental strategic missiles. Western Europe (and the Pentagon) began to see in adequate U.S. forward-based systems an essential escalatory link that coupled the United States to Europe, which would be weakened if the Soviet Union should obtain preponderance over NATO in the area of intermediate-range nuclear missiles.

Haslam is particularly skillful in describing the large diplomatic stakes (and the need on all sides to negotiate not only with one another but with their respective military and foreign policy establishments), which lent such political urgency to the issue and which far surpassed the technical considerations attached to the qualitative or quantitative military balance. In fact, the "objective" geostrategic imperatives of all parties involved and the institutional and bureaucratic pressures that drove or constrained the development, deployment, and ultimate removal of these weapons are presented so compellingly that one is surprised to see Haslam highlight on some occasions the ideological, rather than the geostrategic, sources of the East-West conflict.

The Federal Republic, for reasons of geography, history, and its special status in the Western alliance—full of nuclear weapons, devoid of nuclear control—played a central role in the INF affair, not only because Chancellor Helmut Schmidt had raised the issue in public in the first place but because he was compelled to support NATO's double-track decision against mounting domestic opposition (thereby losing the support of much of his own party and straining the cohesion of the Social Democratic-Free Democratic party coalition) and in very different international circumstances from those within which he had first advocated redressing the Eurostrategic imbalance. Although Schmidt shared the increasingly onerous political responsibility for the first track of the 1979 NATO decision (deployment), he had practically no influence over the second (the arms control negotiations that were to make the first track unnecessary) once the first Reagan administration made clear its distaste for arms control.

Jeffrey Boutwell's *German Nuclear Dilemma* is less concerned with the international dimension of the INF issue than with the domestic response to the deployment of Pershing IIs, such as the Social Democrats' reaction at party conventions, the rise of the German peace movement, and the conflicting attitudes within the Christian Democratic-Christian Social Union on the deployment decision and its ultimate resolution with the INF treaty of 1987. Although handled with competence, these issues are only inadequately placed in the larger diplomatic and military-strategic setting; and the introductory chapter on the evolution of the German nuclear dilemma between 1949 and 1975 is not very illuminating. It is a story often (and better) told before, where all the familiar secondary sources are rejuvenated and one nods in agreement not because the argument is presented so convincingly but because it is so familiar. The short chapter on Schmidt's involvement is flaccid, as well, and has been told with much better insight in articles by Helga Haftendorn and in Barbara D. Heep's *Helmut Schmidt und Amerika* (1990). What we get is a somewhat rotund composite, consisting of a number of more or less connected essays that lack a sustained central theme. Annoying also are the innumerable spelling errors of German sources and concepts, both in the notes and in the text, which are an embarrassment to both author and publisher and raise questions about the author's ability to handle German source materials, as is exemplified by the garbled and misleading translations on page 117.

Ian M. Cuthbertson and David Robertson's *Enhancing European Security* is perhaps the most ambitious of the three. Although at times very technical and forbidding in the use of jargon, this is a work of sophistication, subtlety, and imagination. The authors "intend to investigate ways in which European powers, operating either in conjunction with or even in concert against the superpowers, can diminish the chance of war in Europe" in order to fashion a security policy for Europe that "should be stabilizing military strengths at a level no higher than is actually needed and affordable by the various powers" (pp. 3, 5). The difficult question of how to achieve that aim (made no less urgent or problematic by the revolutionary events of 1989-90) is addressed in this book with a convincing, multilayered argument and selective recommendations that avoid easy solutions. The promise (and problems) of "nonoffensive" defense as well as more aggressive conventional deterrence doctrines are examined with judicious care; and the authors arrive at the conclusion that in Europe the essential prerequisite for the avoidance of war is to diminish the capacity for long-distance invasion and rapid penetration, since "the needs of European nations (with the possible exception of France and the United Kingdom) are for territorial defense. For them, the loss of assets [through arms reduction agreements] needed for invasion cannot hurt" (p. 214).

All three books contribute to our understanding of past arms control measures and how they can in the future contribute to not only the security but the stability of the emerging European political order.

WOLFRAM F. HANRIEDER

University of California, Santa Barbara

Nations and Nationalism since 1780: Programme, Myth, Reality. By E. J. Hobsbawm. Cambridge: Cambridge University Press, 1990. 191p. $19.95.

Personal Identity, National Identity, and International Relations. By William Bloom. Cambridge: Cambridge University Press, 1990. 194p. $39.50.

These two slender volumes come to profoundly contrasting conclusions on the nature of nationalism. Bloom finds that it derives from individual needs for psychological identification, whereas Hobsbawm interprets it as a creature of myth as much as reality, one shaped by the historical era that surrounds it. Both authors touch important dimensions of nationalism, yet in the hands of each it remains a phenomenon that defies full explication.

In the more intriguing of the two volumes, Hobsbawm traces the evolution of nationalism from the French Revolution to about 1950, with a brief concluding chapter on the last half of the twentieth century. Like Hobsbawm's earlier books on bandits and revolutionaries, this one reveals a sprightly wit and a warm sense of humor, perhaps especially because it is based on the Wiles Lectures that Hobsbawm delivered at the Queen's University of Belfast in 1985.

Besides a tone that often seems designed to delight a listening audience, Hobsbawm's work is thoughtful and provocative. Importantly, he reiterates that we cannot take the literary statements of the elite to represent the past feelings of ordinary citizens, revealing his special deference to the sentiments of the rank-and-file. In the face of much of the evidence that he cites, Hobsbawm concludes that at the end of the twentieth century, nationalism "is no longer a major vector of historical development" (p. 163). A scholar concerned with the phenomenon of nationalism for decades, Hobsbawm uses specific cases to jar, surprise, and inform his readers: the founder of the Basque National party who had to learn the Basque language as an adult (p. 119), the query whether patriotism as measured by a willingness to die for the fatherland was really greater for the soldiers of William II or Hitler than for the Hessian mercenaries rented out by their prince in the American Revolution (p. 79), or the interpretation that the militant separatism in Quebec may in fact be "a surrogate for the lost traditional Catholicism" (p. 166).

In contrast to the historical diversity that Hobsbawm finds in nationalist phenomena, Bloom tends to reduce them to the issue of psychological identification. Stressing what he calls "identification theory," he claims that "what it may appear to lack in holism, it makes up for in clarity and parsimony" (pp. 57–58). The argument is that individuals seek psychological security; they find this through nationalism, so that they act in concert in the interests of their national community. While Bloom traces this psychological dimension of nationalism in relation to many eras and issues and relates it quite helpfully to important writing on nationalism by other social scientists, it would be difficult to argue that his thesis on the overwhelming importance of this dimension is entirely convincing.

Bloom's book remains on such a high level of generality that some critics will find it wanting. For example, he notes that Latin was the common language of Christendom in medieval England and France (p. 65), forgetting the various languages and dialects actually spoken at the time by most people in what today are those nations. Some U.S. realists will wince at his contention that they find "that human nature is essentially savage and aggressive" (p. 112). While Argentines would certainly admire this London scholar for referring at times to the Falkland Islands only as the Malvinas (p. 102), most would heartily disagree with his contention that Argentine generals had "no choice but invasion" of the islands in 1982 (p. 94). At the end of the volume, some will reject his conclusion, expressed with admirable candor, that "the notion that it is a glorious thing to die for one's country is a dangerous and terrible idea" (p. 163).

With this level of generality running throughout much of the book, Bloom tends to speak of national polities as undifferentiated aggregates, failing to probe in detail for their component groups that Hobsbawm delights in describing. The width of the literature that Bloom has consulted and integrated is impressive, but readers will find some of his conclusions controversial.

The criticisms that one might level at Hobsbawm's volume differ categorically. Hobsbawm is rich in historical detail but weak on theory, whereas Bloom is quite the reverse. For example, Hobsbawm's intellectual preferences would seem odd to most political scientists. He dismisses the writing of Karl Deutsch on nationalism with a single reference, while he cites the work of Otto Bauer four times, even complaining in a note (p. 2) that "unaccountably," the work of Bauer seems never to have been translated into English.

Despite these contrasts, the two volumes dovetail at some points. Hobsbawm notes that Shakespeare's "propagandist plays about English history" must in fact be considered in terms of modern patriotism (p. 75), while Bloom attempts to explain why a sense of patriotism arose in England and France in the centuries before Shakespeare and the Sun King (pp. 62–71). Both Bloom explicitly (pp. 11, 73) and Hobsbawm less explicitly (pp. 10, 78) argue that we must understand popular attitudes and values associated with nationalism and survey research; in fact, investigating nationalism through survey data should be one of the most fruitful lines of scholarship in the 1990s.

Overall, therefore, both of these volumes repay close reading, Bloom's for his emphasis upon psychological identification and his useful summing up of the social science literature since the 1960s, and Hobsbawm for his awareness of the peculiarities and frequently the ironies of particular historical experiences. If the two books helpfully summarize—as of the end of the 1980s—approaches to the study of nationalism from the perspectives of social psychology and social history, they also suggest, explicitly or implicitly, that we must gather and analyze more empirical data on nationalism in the years ahead.

FREDERICK C. TURNER

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The Insecure Alliance: Energy Crises and Western Politics since 1944. By Ethan B. Kapstein. New York: Oxford University Press, 1990. 257p. $35.00.

Oil has long been a case study for students at the intersection of international political economy and security studies, but few of the resulting books are so informative and well written as *The Insecure Alliance.* Ethan Kapstein delineates how relations within the Western alliance have determined the shape of international oil markets since World War II. The theoretical constructs that he employs will be useful for explaining the politics of international economic relations in many other issue areas.

Kapstein's independent variables are hegemonic resources (referring, of course, to the United States) and the unity-of-alliance purpose, or the collective valued interests, of Western allies. When both hegemonic resources and collective interests characterized NATO politics there was cooperation on energy policy. The book traces cooperation and its sources through the postwar coal crisis (1944–47) and the nationalization of oil company assets in Iran (1951–54). The collective interests of the alliance disintegrated before the United States was marked by decline, which resulted in what Kapstein terms "coercive coordination." Oil policies during the Suez Crisis and the Six Day War are examples of policy coordination that was enforced coercively by the U.S. government.

By the time of the Arab oil embargo in 1973–74, U.S. hegemonic resources were diminished (which is to say that the United States ran out of oil); and in the absence of collective interests, the outcome was conflict. The 2-by-2 matrix of hegemonic resources and alliance unity was completed by the revolution in Iran and the Iran–Iraq War. Western nations shared collective interests, but Kapstein argues that U.S. resources had declined too much to bring about cooperation. "Attempted coordination" was the outcome.

Like most good theories, this one is parsimonious and almost so obvious that the readers are left wondering why they had not thought of it before. It is a thesis that can and should be applied to many other issue areas at the crossroads of security and economics. As with energy, many markets are best understood by examining the distribution of interests and capabilities among advanced industrialized democracies. Many economic issue areas share in common the cycle that Kapstein describes of cooperation, coercive coordination, conflict, and attempted coordination.

In the literature on the international energy market, the predominant theory attributes variations in outcomes to simple changes in supply and demand. Increased demand (due to a growing world economy) and the limited supply of an exhaustible resource are convincing reasons for diminishing cooperation. High prices in the late 1970s then led to conservation and a supply glut, which would explain "attempted coordination." What this sort of explanation misses—and what *The Insecure Alliance* describes—is how the United States set down the rules of the road for the oil market after World War II. Energy cooperation was marked by collusion among consumers to prohibit bilateral deals with producers and to prevent competition, which kept prices down. The framework of meaning by which we interpret outcomes in the oil market was formulated during the postwar coal crisis and can in large measure be explained by the role of energy in the Marshall Aid program.

Kapstein's study is one of the very few places to find a history of these crucial events. It is researched thoroughly (making use of primary materials), clearly formulated, and well-written. I have assigned it at both undergraduate and graduate levels to illustrate theories of international political economy and explore the causes of the recent conflict in the gulf. *The Insecure Alliance* is highly recommended reading.

DAVID E. SPIRO

Columbia University

Human Rights Treaties and the Senate: A History of Opposition. By Natalie Hevener Kaufman. Chapel Hill: University of North Carolina Press, 1990. 256p. $32.50.

Natalie Hevener Kaufman has produced a useful, if problematical, descriptive history of the Senate's failure to give consent to three major UN human rights treaties from 1945 and 1985. Largely by summarizing the documentary record of UN and Senate proceedings and by the use of in-depth analyses of selected issues, she presents an overview sympathetic to the treaties and critical of the critics.

Kaufman, in commendably clear prose, shows that the Genocide Treaty, the Covenant on Civil and Political Rights, and the Covenant on Economic, Social, and Cultural Rights were blocked in the Senate by those who distorted much of their meaning in order to present them as a threat to a sanctified U.S. way of life. Led by a small group of influential lawyers in the American Bar Association (ABA), treaty opponents managed to create an air of controversy around the broad subject of human rights treaties. The treaties supposedly diminished individual rights, encroached on states' rights, promoted world government, enhanced communist influence, put U.S. citizens at risk abroad, threatened the U.S. form of government, and infringed on domestic jurisdiction. That most of these charges were untrue did not diminish their acceptability in Washington.

Kaufman finds great stability in these arguments over time, although she notes Senate consent to several human rights treaties: the Supplemental Slavery Convention (ratified in 1967), the Convention on the Political Rights of Women (1975), and finally the Genocide Treaty (1986). She argues that because of the general aura of controversy, these three ratifications do not portend acceptance of other human rights treaties. She believes these three were accepted because they entailed symbolic gestures that did not impinge greatly on U.S. society. She wrote her book before the Senate gave its consent to the Convention against Torture (1990).

While Kaufman has systematically covered a neglected subject, this work is likely to be problematical to advanced students and specialists in the field. Much of her coverage is conventional—for example, that the movement for a "Bricker Amendment" to the Constitution, reducing the authority of the executive under the treaty clause, was a reaction to the UN human rights treaties and that the Soviet Union did not greatly affect the content of the UN human rights treaties. She does present some new material about the internal politics of the ABA during the era of the early cold war and McCarthyism.

Her view that all three of the UN human rights treaties studied were basically compatible with dominant values in U.S. society is incorrect. The Covenant on Economic, Social, and Cultural Rights is fundamentally different from the conception of legal rights practiced traditionally in the United States. The United States and its constituent states have never recognized a fundamental personal right to adequate nutrition, health care, or shelter, to which governments are obligated to respond.

Rather, such things as food stamps, health service, and shelter are considered benefits, not rights, that the various governments may provide if they so choose. Kaufman's argument that by making socioeconomic rights subject to progressive implementation over time, the executive had answered its critics, misses the point that progressive implementation of something fundamentally different does not make it any less fundamentally different.

Kaufman's irritation over the fact that treaty opponents threw up precise and detailed legal barriers to the policy objective of promoting internationally recognized human rights indicates at best a confusion, at worst a naïveté, about the relation of politics and law. Aside from the fact that she never clarifies the difference between politics and law, Kaufman fails to acknowledge that politics (i.e., a process of making policy involving exercise of power) frequently takes the form of legal debate (i.e., a debate on the precise language of rules). The more determined an intense minority, the greater the likelihood of legalistic maneuvering.

Finally, there are some problems of documentation. Kaufman's argument about the stability of critical views of the UN covenants over time is based on interviews with a total of 10 Senate aides, including only 3 Democrats. This obviously inadequate ground for generalization makes it difficult to be certain about the reliability of this part of her argument. The form of Senate consent to the Convention against Torture, including the dropping of a reservation about means of supervision that had been offered by the Reagan administration, suggests a further decline in the influence of the Jesse Helms coalition that continues to oppose UN human rights treaties on principle. Relatedly, it would have been helpful to have some documentation on exactly why the Reagan administration came to lobby for consent to ratification of the Genocide Treaty.

In sum, this is a useful overview for the beginning student, systematically summarizing in clear prose much UN and Senate debate. Readers interested in this subject would do well to compare the coverage of Senate consideration of the Genocide Convention found in Lawrence J. LeBlanc's *United States and the Genocide Convention* (1991). Prospects for U.S. adherence to the UN covenants and other human rights treaties are discussed in the first 150 pages of the *George Journal of International and Comparative Law* 20 (1990).

DAVID P. FORSYTHE

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War and Democracy. By Peter T. Manicas. Cambridge, MA: Basil Blackwell, 1989. 491p. $70.00.

Some of the troubling concerns that will linger long after U.S. forces return from the Persian Gulf will be in the realm of political theory. This is the first major conflict in which the United States deployed an all-volunteer force, with implications for the distribution of group as well as individual risks that began to be debated even as the troops embarked. It compels a reexamination of relationships between the obligations and benefits of citizenship. And, like the expansion of commitments in Vietnam, it raises anew questions about how an ostensible democracy makes a collective choice for war: What decision rules are to be used? Whence do they derive? What are the bounds of their legitimacy? We—academics, policy makers, and citizens—will need all the help we can get as we grapple with such questions.

There is much indirect help in this sweeping, extraordinarily erudite exemplar of purposive historical scholarship. By tracing the links between the evolution of the conduct of war and the theory and practice of democratic governance (generally viewed), Manicas shows how those questions were answered by major political philosophers and in politics from ancient Greece to the United States of World War II. He gives us a good backdrop for the analytic and normative theories we need but falls short, by 45 years and several crucial developments, of directly applicable insights. Ending his analysis when he does, he avoids critical problems for connecting war and democracy arising from the juxtaposition of modern (particularly nuclear) warfare and (at least in larger polities) the attenuation of democracy through tenuous representation. By failing to touch upon the tensions that have inhered in those connections for two generations or with institutional means through which they might be ameliorated, he leaves us no better off than we were after confronting the limitations remaining after Robert Dahl's effort to deal with these issues directly (*Controlling Nuclear Weapons*, 1985) or the problems of aggregation to the national level that Benjamin Barber could not tame (*Strong Democracy*, 1984).

What Manicas does show very well in what he acknowledges to be a highly idiosyncratic book is how citizenship was engaged not only in combat but in the decision to go to war. At least by implication, we are forced to confront the import of more recent developments in war making for democratic theory and practice. Historically, the need for more soldiers and material resources for war often drove the extension of citizenship. Through his own brand of resource mobilization theory, Manicas goes beyond Tilly's general connection of war making and governance, to show its fruitfulness across more than two millennia; and he explains the growth and institutionalization of democracy, broadly viewed, through a specific, robust version of such theory. Since it affects the density of the collectivity, not just the lives and treasures of its members, war brings to the fore questions about the norms and institutions that give content to regimes of governance. Crucial aims, decisions, and practice in foreign policy (for which war can serve as a synoptic concept) make confronting what—whose—values and interests are to be protected more urgent than do most domestic issues.

Because war is only one of the state's activities that shape the regime, Manicas frequently digresses into reanalyses of well-known treatments of the relationships between the size, nature, and organization of various types of states, governance, and democratic theory. Some of these text-like asides are predictable; but his rereadings of Machiavelli, Montesquieu and the founding fathers provide fresh, solid, and occasionally controversial, insights.

Restricting concern with more recent connections between the title themes to the first half of this century and the United States misses much, however. There is nothing on the transformation of institutional relationships associated with modern war making—a major concern for Dahl in his 1985 book. Manicas also ignores entirely the prevalent pattern that has emerged across most democracies: the subordination of war making to economic well-being. Aside from manifestations at the international level noted by, among others, Richard Rosecrance (*The Rise of the Trading State*, 1986), there is much evidence that both for grounding citizenship and mobilizing resources, war has yielded to welfare in most European and some other democracies. Many states have contracted out vital military functions through alliance—or have forgone them entirely. In the period since World War II, tensions between democracy and war making have emerged such that regimes holding closely to the former seem to lose the capability to engage effectively in the latter (or the other way around: effective war making makes rigorous adherence to democratic norms difficult or impossible).

Manicas provides valuable, if indirect, clues to what is missing or no longer feasible now. While admiring the sweep and grasp displayed in support of the resource mobilization model, one still laments the failure to note the consequences of the rise of modern weapons or the international politics and political economy of arms transfers (which help to explain the absence of the democratizing impetus of war making in recent times) or to contemplate at all the converse of the causal dynamic suggested by the title: whether, in most cases, mature, less attenuated democracies can move to war.

Only nostalgia and glaring contrast connect this partial, personal, but solid treatment of an important historical theme to current theoretical and policy issues. Its rigorous analyses of relationships between war and democracy can serve as bases for strong, if pessimistic, hypotheses regarding the applicability of democracy to the determination of modern problems of war and peace and the capability of modern democratic states to exercise at least some important military functions effectively. Those with an interest in the origins and history of democratic governance and war in the West will benefit from reading this book; and students of connections between democracy and security in more recent times will find it a broad and firm wall from which to push off for analytic and theoretical leverage.

MARTIN O. HEISLER

University of Maryland

A Century of Debt Crises in Latin America: From Independence to the Great Depression. By Carlos Marichal. Princeton: Princeton University Press, 1989. 283p. $45.00 cloth, $16.50 paper.

Debt and Democracy in Latin America. Edited by Barbara Stallings and Robert Kaufman. Boulder: Westview, 1989. 232p. $29.00 cloth, $14.95 paper.

Lost Promises: Debt, Austerity, and Development in Latin America. Edited by William L. Canak. Boulder: Westview, 1989. 244p. $37.00 cloth, $16.00 paper.

In August 1982, Mexico's finance minister telephoned the U.S. Treasury and announced that his country had no money to repay its loans. That call initiated a debt crisis that would soon engulf Latin America and sub-Saharan Africa and last for a decade. Many of these states had borrowed heavily in the 1970s to sustain rapid growth in the face of rising oil prices. A second oil crisis in the early 1980s proved too much. When export markets collapsed and global interest rates rose markedly, many Third World borrowers faced default.

The ensuing crisis has been marked by economic retrenchment in debtor states and by serious financial losses among creditors. At first, most analysts figured the setback would be sharp but brief. They were wrong. Although interest rates soon fell and the world economy revived, problem debtors did not recover. They were plagued by capital flight, poor export prices, weak domestic investment, and little new capital from foreign sources. Creditors offered them meager relief, at least until the 1989 Brady Plan. Meanwhile, the debtors themselves tried a wide variety of policies, ranging from conventional austerity and devaluation to unilateral limits on debt payments. The prolonged difficulties have become central features of politics as well as economics in the Third World.

William Canak's edited volume, *Lost Promises,* concentrates on economic austerity and its political and social impact in Latin America. Its broad focus is on the shifting international division of labor. Most essays, however, concentrate on domestic dislocations, particularly the impact of austerity measures on real wages and their distribution. It is truly comparative (as are the other two books). Many of its essays range systematically across several debtor states. Typical is Edward Epstein's essay, which examines organized labor's response to austerity in seven countries. His conclusions stress the combined impact of unemployment levels and political control mechanisms on strike activity (including both frequency and violence). John Walton and Charles Ragin conduct a cross-national, statistical comparison of popular protests against austerity.

Several essays, however, simply assert their critical stance, without nuanced interpretation or close consideration of the data and competing hypotheses. Canak's introductory essay provides a framework for the essays that follow (much of it in terms of core-periphery relations and domestic class relations); but he does little to corroborate his sharp critique of international financial institutions. He simply asserts, without qualification or documentation, that multilateral institutions have "functioned . . . to limit the capacity of nationalist and socialist groups to influence state policy" (p. 11). He goes on to argue that the World Bank and International Monetary Fund are focal points for the "global coordination of capital and the direct sanctioning of national regimes" (p. 13). These are familiar arguments, of course, but they are not well elaborated or substantiated here. What, for example, are the processes by which these institutions control private, profit-making corporations? And what is the underlying logic of this coordinated action?

Some of the most interesting essays concentrate on neglected dimensions of the debt crisis. Merilee Grindle, for instance, analyzes how Mexico's rural poor have coped with austerity. As she points out, their responses "occur within the peasant household, not in the public arena, and are often overlooked in the search for more dramatic evidence of collective political protest" (p. 191). Alejandro Portes and Michael Johns consider the polarization of class and space in urban areas. They offer a useful typology of reactions: spontaneous riots, community-based militance, and disengagement. Ralph Hakkert and Franklin Goza survey the demographic consequences of austerity, with close attention to the data problems. They note, for instance, that "most researchers believe that a reconcentration of income occurred in Latin America as a result of the crisis" but quickly add that "it is difficult to obtain unquestionable results indicating this" (p. 76). They are particularly good in showing adjustments within the labor force, including the rise of the informal sector, increased participation by women and older workers, and shifting patterns of migration.

What effect have these pervasive austerity measures had on the forms of governance and on state-society relations? These fundamental questions dominate Barbara Stallings and Robert Kaufman's fine edited volume, *Debt and Democracy in Latin America.* Why, they ask, have so many Latin American states sustained their new democracies under such adverse economic conditions? Essays by Rudiger Dornbush and Riordan Roett provide useful background on creditor policies and the international economic context. Dornbush, writing before the Brady Plan, argues strongly for unilateral action by debtors to limit annual repayment burdens. Roett correctly observes that West European and Japanese policy makers have done little to respond to the debt crisis, deferring to Washington, whose own policies have been slow and modest. This description and prescription are not accompanied, however, by a sustained effort to explain the rationale and limits behind U.S. policy. Jeff Frieden and Sylvia Maxfield, in separate essays, concentrate on the distributional impact of the crisis within debtor states. Their essays show how borrowing in the 1970s promoted rapid expansion of domestic financial sectors and greater industrial concentration, led by state-owned firms. The debt crisis reversed both trends. Paul Drake's superb chapter asks why the debt crisis in the 1930s undermined democracies and produced stable dictatorships while the crisis of the 1980s produced exactly the opposite result. Simply posing the question—and posing it so starkly—is a contribution to the debate. But Drake also offers some intriguing and plausible answers. His basic argument is that the initial shock of a debt crisis undermined the political support for existing regimes, whatever their type. He adds that the debt crisis of the 1980s was especially damaging to military dictatorships, which had relatively narrow social support and relied heavily on their claims of sustaining order and fostering economic growth. The initial shock of debt crisis undermined these most basic claims to political legitimacy. "It is that initial, sudden drop in resources, production and incomes that is most disruptive to governments," Drake concludes: "The subsequent slow, grueling process of adjustment and recuperation is much less destabilizing" (p. 55). The volume is filled with valuable essays on the politics of that grueling adjustment: Robert Kaufman on Mexico, Eul-Soo Pang on Brazil, Joan Nelson on Costa Rica, Carol Wise on Peru, Barbara Stallings on Chile, and Ian Roxborough on Argentina and Brazil.

Drake's essay also reminds us that Latin American debt crises are recurrent. Carlos Marichal's learned book details these cycles of overborrowing and default from the nineteenth century through the Depression of the 1930s. Its achievement is to explore the complex interaction between lenders and borrowers. This is a rare book, moving easily between the domestic politics of Latin debtors and the anxious bondholders and issuing houses in London. The case studies are well documented; the text and notes reveal an understanding of both credit markets and debtors' politics. Drawing on this background, Marichal provides a rich account of such major events as the 1890s Baring crisis, including a lengthy discussion of the prior "loan frenzy in the Rio de la Plata." Marichal is weakest, I think, in defending his general argument that the fate of Latin debt is determined mainly by cyclical conditions in world credit markets. His case studies often suggest otherwise: that loans failed because the funds were diverted or used for consumption or because the underlying projects failed to produce real returns high enough to cover the interest charges with their steep risk premiums. That is an old story, forgotten in the 1970s but remembered ruefully today.

CHARLES LIPSON

University of Chicago

Turbulence in World Politics: A Theory of Change and Continuity. By James N. Rosenau. Princeton: Princeton University Press, 1990. 480p. $55.00 cloth, $14.50 paper.

All of us recognize that the international system is currently undergoing profound changes. Exactly *how* substantial is the subject of this work, which advances a theory to support the claim that the present transformation has been unequaled since the beginning of the modern nation-state system following the Treaty of Westphalia in 1648. This book's daring matches the breadth of its subject.

Rosenau postulates that the present international political system depends on three key parameters whose boundaries have contained periods of fluctuation and change for over three hundred years. Since World War II the parameters have been shattered by a series of developments—some having to do with technology (particularly the microelectronic revolution in communication), other factors like the emergence of transnational issues that nation-states have not been able to solve. The result has been the current period of turbulence, a concept the author has borrowed from physics to describe the complexity and dynamism of extensive changes in this new era of world politics.

The three overwhelmed parameters that have unleashed the present turbulence are the orientation and skills of individual citizens worldwide (the micro parameter), the distribution of power in the collectivities of the global system (the structural, or macro, parameter), and the authority relations between aggregated individuals and collectivities (the relational parameter). A trigger to the rupture of all three parameters has been a recent worldwide shift by individuals toward greater adaptive learning and away from habitual, rote behavior. This shift has led to a breakdown in the individual orientation parameter. In turn, the collapse of this limit (together with other factors) has transformed the authority relationship between people and collectivities. In exchange for their loyalty and compliance, people are now more inclined to demand that collective entities respond to their needs. When states have been increasingly unable to solve problems and meet human needs, the distribution of power (the structural parameter) also has been exceeded.

One consequence of the collapse of the structural parameter has been the emergence of a multicentric world consisting of thousands of nonstate, nonsovereign global actors. According to Rosenau this multicentric world coexists in a nonhierarchical relationship with the state-centric system.

Another characteristic of the current era of global turbulence consists of strong, simultaneous pressures for both centralization and decentralization. Part of the decentralization trend is toward "subgroupism," in which individuals shift some of their loyalties from the nation-state to collectivities closer to them in which there is more possibility for meaningful participation and monitoring of the entity's performance.

Realists, neorealists, postmodernists, behavioralists, and simple, unabashed skeptics will find much to challenge them in this work. Rosenau postulates, in effect, that the international system has divided like a biological cell into a state-centric and a multicentric world. The evidence for this division is, at best, anecdotal—alleged anomalies and discontinuities. To demonstrate a transformation of the international system requires more. From the perspective of the existing system paradigm, can we be confident that recent developments pose more serious challenges than those in the past without systematic comparison? What has the state-centric system successfully explained that it now does not explain?

Does the existence of numerous nonstate actors confirm that the state system itself underwent a transformation or that a number of quasi-separate systems have developed alongside it? (Some nonstate systems like Catholicism or Islam have been present for quite some time.) If a parameter is a boundary or limit, at what point is it exceeded? For example, the shift of the world's population to a greater use of adaptive learning instead of behavior based on habit is essential to explain the collapse of the micro parameter—individuals' orientations and skills. How much of a shift away from habit dependent behavior causes a breakdown in the parameter? Put another way, how could researchers test or falsify the Rosenau assertion? Numerous other questions abound. The multicentric counterpart to the state-centric system is called a world, not a system. Analytically, what is a "world"? Why is it not a system with its own parameters and rules of interaction? If more than one world can exist simultaneously, why only two? What about the developing world in all this analysis? Are the patterns of subgroupism growing in Third World countries? Have there been significant shifts among people there in adaptive behavior? Despite such questions and challenges, there is much to admire and praise in this book.

The multilevel theory is strikingly bold and novel for making human capabilities the engine of change. Creativity enlivens the approach to other problems as well. For example, role scenarios and action scripts are introduced to infuse the dynamics that so often have been missing in previous work on role analysis. Although the breaking points of the three parameters remains unspecified, other propositions are advanced that could stimulate important empirical research. James Rosenau fears that scholars in international politics have become trapped in their own conceptual jails. If his new work triggers substantial debate and encourages at least a few analysts to conduct empirical research to check some of his testable propositions, then his provocative venture will undoubtedly succeed in its basic purpose.

CHARLES F. HERMANN

Ohio State University

Hans J. Morgenthau and the Ethics of American Statecraft. By Greg Russell. Baton Rouge: Louisiana State University Press, 1990. 258p. $27.50.

In his most recent addition to Kenneth Thompson's Political Traditions in Foreign Policy series, Greg Russell offers an extensive and wide-ranging analysis of the contribution of Hans J. Morgenthau to U.S. foreign policy. It is extensive because it covers all of Morgenthau's work. It is wide-ranging because the author coherently integrates a variety of important issues in contemporary international relations. Ethics, international law, U.S. foreign policy, and the theory of realism are all discussed under the umbrella of Morgenthau's political philosophy. Russell begins from the position that Morgenthau's realism evolved away from the European tradition of *raison d'état* and steered U.S. statecraft toward a new political philosophy. This new realism diverged from its European *raison d'état* heritage in two important and often overlooked ways. First, Morgenthau's realism did not profess autonomous ethics for state behavior. Second, his realism incorporated transcendent moral principles in the practice of statecraft. Thus, Morgenthau's realism engaged neither the absolute morality of idealism nor the absolutist principles of state interests driven by *raison d'état.* What then, is Morgenthau's realism?

To understand Morgenthau's political ethics, the author endeavors to place him in multiple contexts but at the same time painstakingly distinguishes his ideas from others. He places his ideas in the historical context of the European tradition of *raison d'état,* the contemporary postwar setting of U.S. foreign policy with such contemporaries as Lippman, Kennan and Niebuhr, and the philosophical context of classical liberalism. All this, however, helps only to frame the ideas. While the analysis is greatly informed by the classical political theory that Russell brings to bear on his reading of Morgenthau, the author is not successful in contriving the dual straw men of idealism and *raison d'état* to highlight Morgenthau's uniqueness. Russell compares Morgenthau with his contemporaries; but however fascinating the comparison between Lippman, Niebuhr, Kennan and Morgenthau, this micro analysis offers little in the way of systematic extension of the *raison d'état*-idealism duality. The result is a work classically inspired and adeptly argued, but without an architecture. Considerably more time and systematic thought is given to Morgenthau vis-à-vis *raison d'état* than to his reactionary roots in idealism. Still missing in this intellectual puzzle are the details of Morgenthau's political ethics.

Morgenthau's realism is based upon the moral use of power. In dialectical opposition, moral principles stand up against each other. For Morgenthau, moral judgment means choosing the least evil of several expedient actions. All politics contain issues of morality, but it is individual choice that makes the political actor inhere to the moral responsibility implict in the choice. These ideas have evolved dialectically from a reaction to both *raison d'état* and idealism. To that extent, Morgenthau's ideas reflect an important departure in the intellectual discourse on political ethics. But the author does not sufficiently explore Morgenthau as reactionary. In addition, the ethical relativism that still plagues Morgenthau's political philosophy is ignored. Since the essential contradictions of his politics of moral expedience are never fully brought to light, the reader is left with an incomplete rendering of Morgenthau, one that somewhat overstates his uniqueness in the intellectual history of realism and unwittingly accepts his propositions in an effort to distinguish his ideas from those of the idealists. There is a serious omission here of any critical review of Morgenthau's political ethics. One could, for example, easily interpret Morgenthau as a Machiavelli with a different *necessita* requiring a different kind of *virtu.*

Students of political theory, international relations, and U.S. foreign policy will find much to commend this book. It is a comprehensive overview of Morgenthau's ideas, philosophy, and ethics and his critical view of U.S. idealism, international law, and organization. It integrates widely read and well-known works such as his *Politics among Nations* (1949) with lesser-known and less accessible materials. The book is a wonderful exercise in comparison of fundamental assumptions about the nature of man, knowledge, and the state. The first two chapters, in particular, offer a valuable contribution to political theory in international relations. Despite its uncritical and defensive tone, the book will also serve as a good supplementary text to Morgenthau's published work for courses on U.S. foreign policy. Many issues salient to contemporary U.S. foreign policy are brought together in chapter 5, especially notable for its discussion of Morgenthau's views on nuclear deterrence, arms control, and disarmament. Overall, the book displays a good blend of theory and practice woven into the text, which gives some color to Morgenthau's ideas. And while the book does not flesh out Morgenthau's political ethics in any detail, it does succeed in placing Morgenthau's politics and policy in the context of Western political thought.

LOIS W. SAYRS

University of Iowa

American National Interest: Virtue and Power in Foreign Policy. By Karl von Vorys. New York: Praeger, 1990. 276p. $39.95.

War, Peace, and Victory: Strategy and Statecraft for the Next Century. By Colin S. Gray. New York: Simon & Schuster, 1990. 442p. $24.95.

Karl von Vorys, in *American National Interest,* and Colin S. Gray, in *War, Peace, and Victory,* are concerned with questions that have long preoccupied students and practitioners of U.S. foreign policy: What goals and priorities are most desirable? Which goals are being pursued? What rationale has been developed and applied for the attainment of goals? What are the uses, misuses, and abuses of the nation's power in the pursuit of goals?

Neither author could have anticipated the crisis and resulting war that would be triggered by the Iraqi invasion of Kuwait in August 1990. However, far from rendering their arguments obsolete, these events underscore the importance of addressing the issues that the two authors discuss.

The Gulf crisis provides an important test of Gray's central thesis: "At the most basic level of all, this book is about winning in world politics. It is about how to succeed in peace and crisis by deterrence, and—if deterrence fails—about how to win wars for the purpose of securing the goals set as war aims" (p. 12). Gray's advice appears solid.

The Iraqi invasion of Kuwait can also be seen as highlighting the questions that most concern von Vorys: What are America's genuine national interests? How can a policy consensus be forged and sustained? My guess is that recent events in the Middle East would lead von Vorys to expand on his concluding argument: "If we are determined to take an active role in settling regional disputes . . . we shall have to do more than be able to respond militarily to crises. It is far better to prevent them by investing in . . . political stability" (p. 260).

The authors share realist assumptions about the continuing importance of the nation-state as the key actor in world politics and about the centrality of the struggle for power among states. Yet neither author embraces classical or neorealist positions uncritically, and neither is content simply to rehash old controversies. Each is sensitive to the dramatic changes that have occurred in international and domestic environments since World War II, for example; and both recognize that a new post-cold war era calls for a critical reexamination of prevailing policy assumptions.

Von Vorys argues that "our national interest now includes . . . a stable international order. . . . We have reached the age of global interdependence" (pp. 149-50). He finds that "the most daunting challenge to our foreign policy and our democratic institutions" is a major reorientation of the thinking of U.S. citizens to accommodate the global pattern of human development and the role of the United States within this broader context (p. 151). Gray, in turn, challenges what he sees as a widespread readiness to shelve serious strategic thought, on the grounds (symbolized in Gray's view by the "end-of-history" thesis expounded by Francis Fukuyama) that "the Cold War has been won and a historical corner definitively turned" (p. 10). Gray himself clearly is in the tradition of realist predecessors such as Bernard Brodie and Arnold Wolfers; but Gray reserves some of his most devastating criticism for neorealists who would equate national power with military capability and/or technological prowess, who would neglect the relation of power to purpose, and who ignore or oversimplify the lessons of history and the fundamentals of geography.

Both authors recognize the magnitude of the transformation that has been occurring in the Soviet Union and Eastern Europe, and both are perceptive enough (considering that both completed their works, presumably, in 1989) to caution readers that the transformation might well bring new instability and a new set of policy problems.

Although the authors have in common the concerns that have been noted, each has his own particular preoccupations, reflecting in large part areas of emphasis in his career experience. Von Vorys argues that the U.S. public needs to pay more attention to Asia (although his own attraction is directed almost exclusively to the most unstable and poverty-stricken portions of Asia, rather than to the newly industrialized countries). He also focuses on instability and economic distress in sub-Saharan Africa, seeing this region also as one whose problems bear incorporation into the U.S. calculus of vital interests.

Gray's book focuses almost entirely upon the policies and problems of the United States and the European powers, past and present, with little attention to other regions and states. Such a focus is common to the vast majority of major strategic works that have been published in English, an excellent guide to which is provided in Gray's selected bibliography.

Scholars who engage in strategic research or teaching surely will add Gray's book to their lists of "must" reading. It is a highly original study, carefully reasoned but often provocative. Its value to those who work in this field is enhanced by an abundance of discursive notes, which expand on various topics and provide a commentary on relevant literature. The book has the additional virtues of clarity of organization and expression, and subtlety and sophistication of argument. The same cannot be said of the book by von Vorys. The latter author's heavy reliance on clichés, homey aphorisms, and excessive colloquialism makes it unlikely that many prospective readers will make it beyond the first chapter.

JOHN P. LOVELL

Indiana University

MICHIGAN MICHIGAN MICHIGAN

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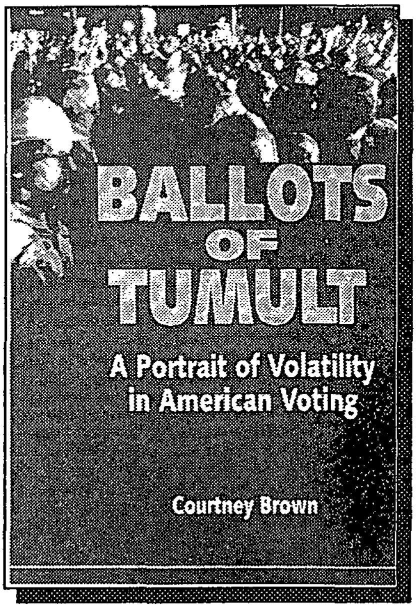

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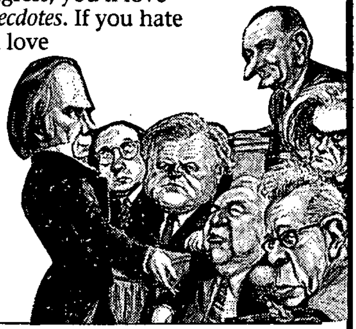

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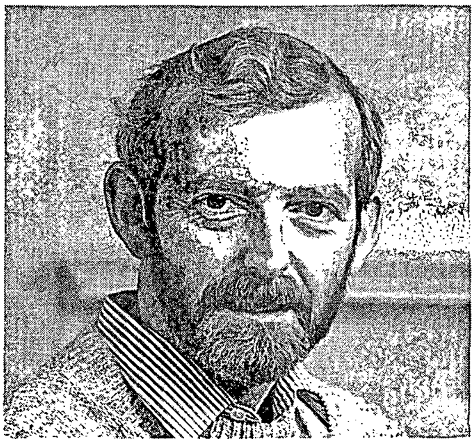

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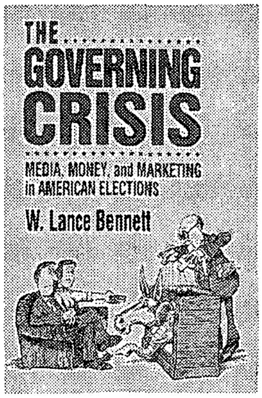
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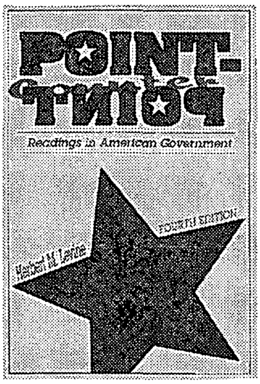
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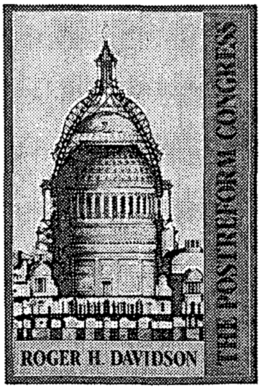
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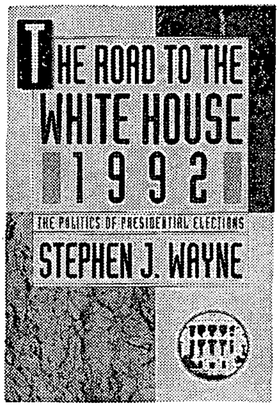
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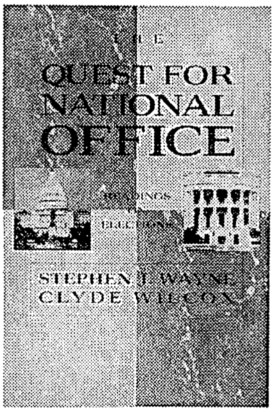
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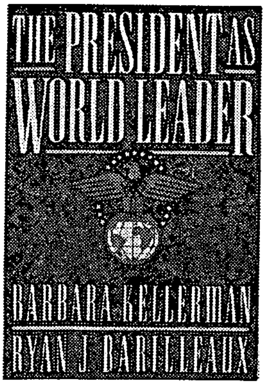
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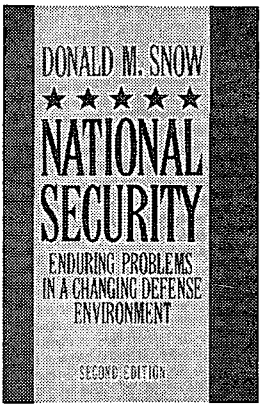
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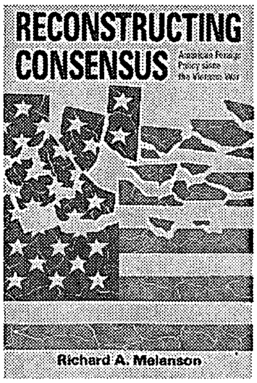
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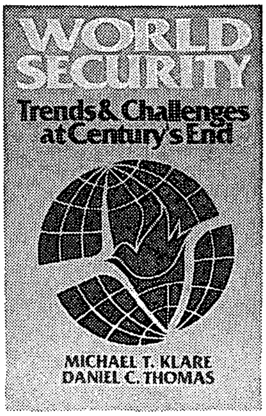
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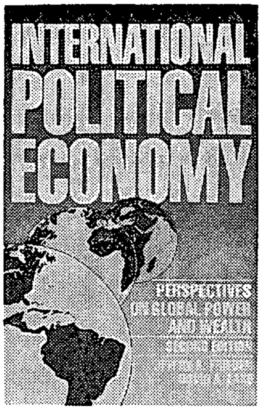
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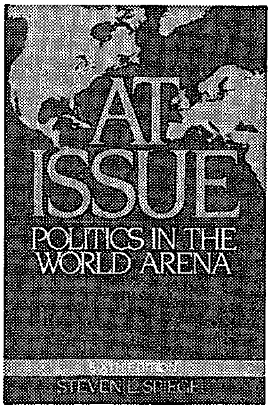
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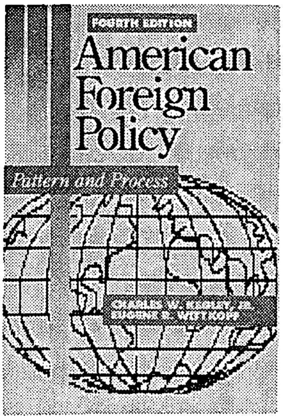
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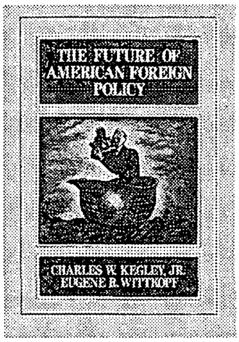
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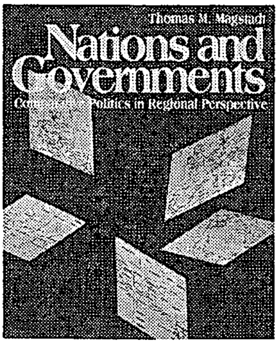
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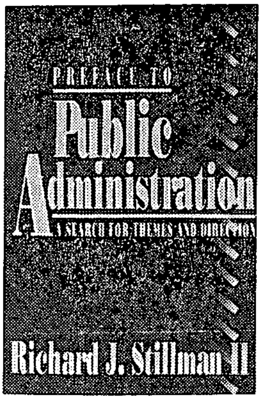

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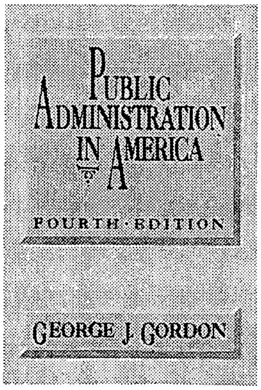

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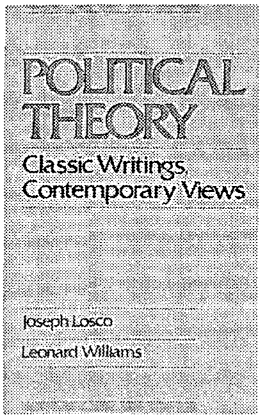

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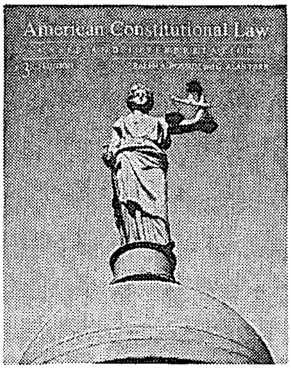

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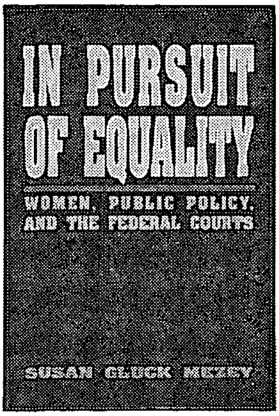

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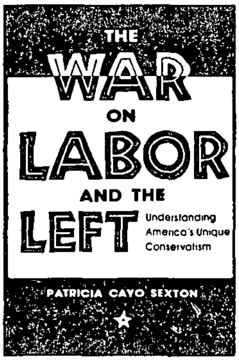

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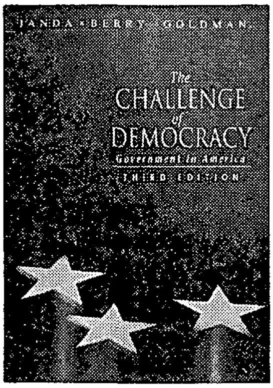

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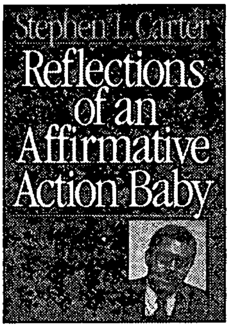

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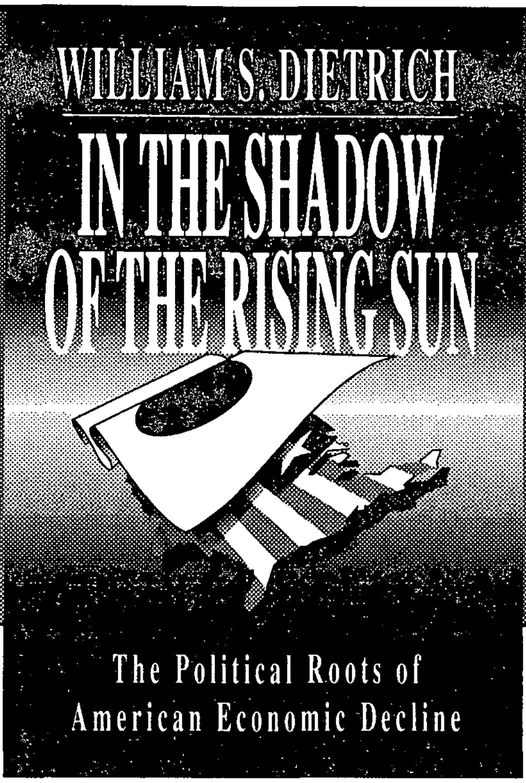

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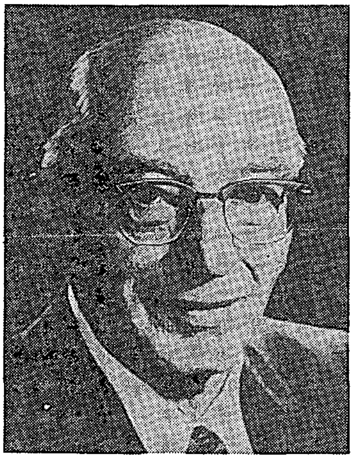

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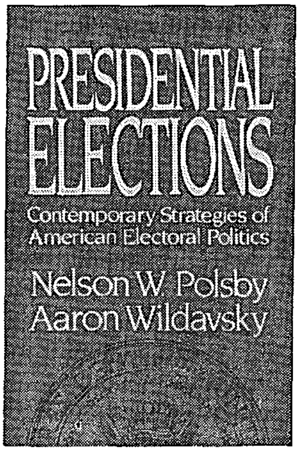

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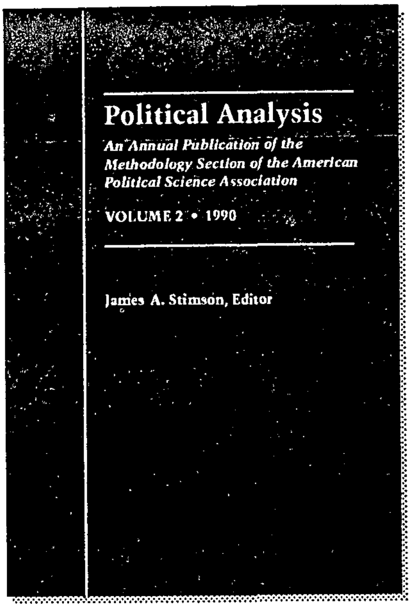
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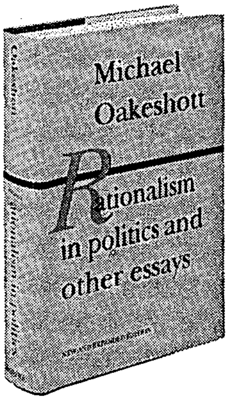

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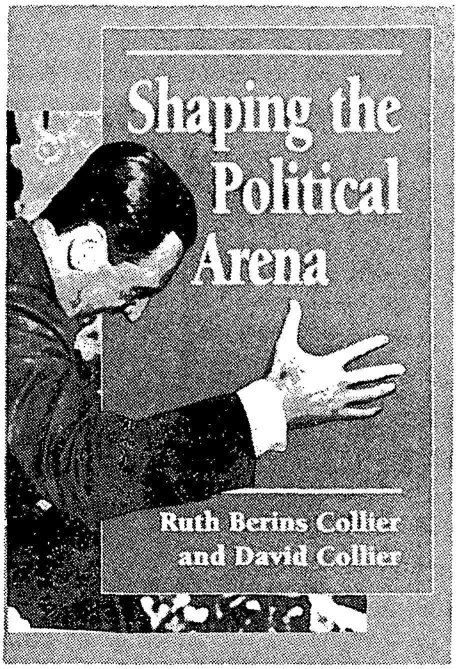
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